SCIENCE

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THE LOGICAL BASIS OF THE SANITARY POLICY OF MOSQUITO REDUCTION.[1]

THE great science of preventive medicine is often called upon to consider new policies of public sanitation, which, whether they ultimately prove successful or not, are always of profound interest and importance to mankind. Quite recently a new measure of this kind has been proposed, which in the opinion of many promises to rank with house sanitation and preventive inoculation as a means of saving human life on a large scale. Unfortunately, its value has not yet been clearly demonstrated—with the result that it is not being employed as largely as some of us hoped would be the case. I feel, therefore, that I can not better acknowledge the honor you have done me in inviting me to address you to-day than by attempting to discuss this important theme—in the hope that the discussion may prove profitable to the cause of public health. The new sanitary policy to which I refer is that which aims at the reduction of disease-bearing insects, especially those which are the disseminating agents of malaria, yellow fever and filariasis.

I presume that it is scarcely necessary to discuss the evidence which has established the connection between various insects and arthropods and many diseases of man and of animals. The fact that the pathogenetic parasites which produce those great scourges of the tropics just mentioned are carried by gnats is now too well known to require reiteration. It is necessary only to

[1] Read at the International Congress of Arts and Science.

remind you that the gnat acts as an intermediary, becoming infected when biting infected persons and, some weeks later, infecting healthy persons in its turn—the parasite passing alternately from insect to man. The hypothesis that the infection in these diseases may be produced in any other manner than by the bite of gnats has not been justified by any recorded experiments or by any substantial arguments; and we may, therefore, assume for the present that if we could exterminate the intermediary agents, the gnats, in a locality, we could also exterminate there the diseases referred to. But here we enter upon ground which in the opinion of many is much less secure. While some believe in the possibility of reducing gnats in given localities and consider that the point has been proved by experiment, others are much more sceptical and hold that the experiments were not sound. This state of uncertainty naturally causes much hesitation in the adoption of measures against gnats, and, therefore, possibly a continued loss of life by the diseases occasioned by them; and I, therefore, propose to sift the matter as carefully as time will allow.

In the first place, we should note that experiments made in this connection have not been very satisfactory, owing to the fact that no accurate method has yet been found for estimating the number of gnats in any locality. We can express our personal impressions as to their numbers being small or large; but I am aware of no criterion by which we can express those numbers in actual figures. We can not anywhere state the exact number of mosquitoes to the square mile or yard, and we can not, therefore, accurately gauge any local decrease which may have resulted from operations against them. A method of doing this may be invented in the future; but for the present we must employ another means for resolving the problem—one which has given such great results in physics—namely, strict logical deduction from ascertained premises.

As another preliminary we should note that mosquito-reduction is only part of a larger subject, namely, that of the local reduction of any living organisms. Unlike particles of matter (so far as we know them) the living unit can not progress through space and time for more than a limited distance. The diffusion of living units must, therefore, be circumscribed—a number of them liberated at a given point will never be able to pass beyond a certain distance from that point; and the laws governing this diffusion must be the same for all organisms. The motile animal is capable of propelling itself for a time in any direction; but even the immotile plant calls in the agency of the winds and waters for the dissemination of its seeds. The extent of this migration, whether of the motile or the immotile organism, must to a large degree be capable of determination by proper analysis; and the logical position of the question of local reduction depends upon this analysis.

The life of gnats, like that of other animals, is governed by fixed laws. Propagation can never exceed, nor mortality fall below, certain rates. Local conditions may be favorable either to the birth rate or to the death rate; and the local population must depend upon the food supply. Diseases, predatory animals, unfavorable conditions and accidents depress the density of population; and in fact local reduction, that is, artificial depression of the density of population, practically resolves itself into (*a*) direct destruction and (*b*) artificial creation of unfavorable conditions.

Let us now endeavor to obtain a perfectly clear picture of the problem before us by imagining an ideal case. Suppose that we have to deal with a country of indefinite extent, every point of which is

equally favorable to the propagation of gnats (or of any other animal); and suppose that every point of it is equally attractive to them as regards food supply; and that there is nothing, such for instance as steady winds or local enemies, which tends to drive them into certain parts of the country. Then the density of the gnat population will be uniform all over the country. Of course, such a state of things does not actually exist in nature; but we shall nevertheless find it useful to consider it as if it does exist, and shall afterwards easily determine the variations from this ideal condition due to definite causes. Let us next select a circumscribed area within this country, and suppose that operations against the insects are undertaken inside it, but not outside it. The question before us is the following: How far will these operations affect the mosquito density within the area and immediately around it?

Now the operations may belong to two categories—those aimed at killing the insects within the area, and those aimed at checking their propagation. The first can never be completely successful; it is in fact impossible to kill every adult winged gnat within any area. But it is generally possible to destroy at least a large proportion of their *larvæ,* which, it is scarcely necessary to remind you, must live for at least a week in suitable waters, and which may easily be killed by larvacides, or by emptying out the waters, or by other means. This method of checking propagation consists, in the case of these insects, of draining away, filling up, poisoning or emptying out the waters in which they breed. Obviously the ultimate effect is the same if we drain away a breeding pool or if we persistently destroy the larvæ found in it; though in the first case the work is more or less permanent, and in the second demands constant repetition. If we drain a breeding area we tend to produce the same effect at the end of a year as if we had destroyed as many gnats as otherwise that area would have produced during that period. Thus, though we can not kill all mosquitoes within an area, even during a short period, we can always arrest their propagation there for as long as we please, provided that we can obliterate all their breed waters or persistently destroy all their larvæ—which we may assume can generally be done for an adequate expenditure. We must, therefore, ask what will be the exact effect of completely arresting propagation within a given area under the assumed conditions?

The first obvious point is that the operation must result in a decrease of mosquitoes. If we kill a single gnat there must be one gnat in the world less than before. If we kill a thousand every day there must be so many thousands less at the end of a given period; and the arrest of propagation over any area, however small, must be equivalent to the destruction of a certain number of the insects. But this does not help us much. It may be suggested that, after the arrest of propagation over even a considerable area, the diminution of mosquitoes within the area remains inappreciable. What is the law governing the percentage of diminution in the mosquito density due to arrest of propagation within an area?

The number of gnats (or any animal) within an area must always be a function of four variables, the birth rate and death rate within the area, and the immigration and emigration into and out of it. If we could surround the area by an immense mosquito bar, the insects within it (after the death of old immigrants) would consist entirely of native insects; on the other hand, if we arrest propagation, the gnat population must hereafter consist entirely of immigrants. The question, therefore,

resolves itself into this one: What is—what must be—the ratio of immigrants to natives within any area? What factors determine that ratio?

Ceteris paribus, one factor must be the size of the area. If the area be a small one, say of ten yards radius, suppression of propagation will do little good, because the proportion of mosquitoes bred there will be very small (under our assumed conditions) compared with those which are bred in the large surrounding tracts of country, and which will have no difficulty in traversing so small a distance as ten yards. But if we completely suppress propagation over an area of ten miles radius, the case must be very different—every gnat reaching the center must now traverse ten miles to do so. And if we increase the radius of the no-propagation area still further, we must finally arrive at a state of affairs when no mosquitoes at all can reach the center, and when, therefore, that center must be absolutely free from them. In other words, we can reduce the mosquito density at any point by arresting propagation over a sufficient radius around that point.

But we now enter upon more difficult ground. How large must that radius be in order to render the center entirely mosquito-free? Still further, what will be the proportion of mosquito reduction depending upon a given radius of anti-propagation operations? What will be that proportion, either at the center of operations, or at any point within or without the circumference of operations? The answer depends upon the distance which a mosquito can traverse, not during a single flight, but during its whole life; and also upon certain laws of probability, which must govern its wanderings to and fro upon the face of the earth. Let me endeavor to indicate how this problem, which is essentially a mathematical one of considerable interest, can be solved.

Suppose that a mosquito is born at a given point, and that during its life it wanders about, to and fro, to left or to right, where it wills, in search of food or of mating, over a country which is uniformly attractive and favorable to it. After a time it will die. What are the probabilities that its dead body will be found at a given distance from its birthplace? That is really the problem which governs the whole of this great subject of the prophylaxis of malaria. It is a problem which applies to any living unit. We may word it otherwise, thus—suppose a box containing a million gnats were to be opened in the center of a large plain, and that the insects were allowed to wander freely in all directions—how many of them would be found after death at a given distance from the place where the box was opened? Or we may suppose without modifying the nature of the problem that the insects emanate, not from a box, but from a single breeding pool.

Now what would happen is as follows: We may divide the career of each insect into an arbitrary number of successive periods or stages, say of one minute's duration each. During the first minute most of the insects would fly towards every point of the compass. At the end of the minute a few might fly straight on and a few straight back, while the rest would travel at various angles to the right or left. At the end of the second minute the same thing would occur—most would change their course and a very few might wander straight on (provided that no special attraction exists for them). So also at the end of each stage—the same laws of chance would govern their movements. At last, after their death, it would be found that an extremely small proportion of the insects have moved continuously in one direction, and that the vast majority of them

have wandered more or less backward and forward and have died in the vicinity of the box or pool from which they originally came.

The full mathematical analysis determining the question is of some complexity; and I can not here deal with it in its entirety. But if we consider the lateral movements as tending to neutralize themselves, the problem becomes a simple one, well known in the calculus of probabilities and affording a rough approximation to the truth. If we suppose that the whole average life of the insect contains n stages, and that each insect can traverse an average distance l during one such stage or element of time, then the extreme average distance to which any insect can wander during the whole of its life must be nl. I call this the limit of migration and denote it by L, as it becomes an important constant in the investigation. It will then be found that the numbers of insects which have succeeded in reaching the distances nl, $(n-1)l$, $(n-2)l$, etc., from the center will vary as twice the number of permutations of $2n$ things taken successively, none, one, two, three at a time, and so on—that is to say, as the successive coefficients of the expansion of 2^{2n} by the binomial theorem. Suppose, for convenience, that the whole number of gnats escaping from the box is 2^{2n}—a number which can be made as large as we please by taking n large enough and l small enough—then the probabilities are that the number of them which succeed in reaching the limit of migration is only 2; the number of those which succeed in reaching a distance one stage short of this, namely, $(n-1)l$, is $2.2n$ of those which reach a stage one shorter still is

$$2\frac{2n(2n-1)}{2\,!}$$

and so on. Hence the whole number of gnats will be found arranged as follows:

Distance from center	nl	$(n-1)l$	$(n-2)l$	$(n-3)l$	etc.	total.
Number of gnats	$2\ +$	$4n$	$+\,2\frac{2n(2n-1)}{2\,!}$	$+\,2\frac{2n(2n-1)(2n-2)}{3\,!}$	$+$ etc. $=$	2^{2n}.

It, therefore, follows from the known values of the binomial coefficients that if we divide the whole number of gnats into groups according to the distance at which their bodies are found from the box, the probabilities are that the largest group will be found at the first stage, that is, close to the box, and that the successive groups, as we proceed further and further from the box, will become smaller and smaller, until only a very few occur at the extreme distance, the possible limit of migration. And the same reasoning will apply to a breeding pool or vessel of water. That is, the insects coming from such a source will tend to remain in its immediate vicinity, provided that the whole surrounding area is uniformly attractive to them.

The following diagram will, I hope, make the reasoning quite clear.

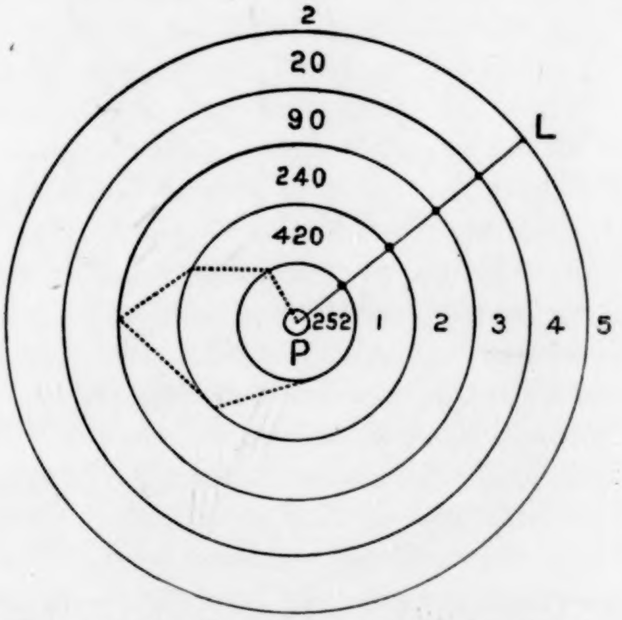

DIAGRAM I. The chance-distribution of Mosquitoes. P, central breeding-pool. L, limit of migration. The numbers denote the proportions of 1,024 mosquitoes starting from P which die at the distances 1, 2, 3, 4, 5, respectively. The continuous line denotes a continuous migration always in one direction; the dotted line, the usual erratic course.

We suppose that 1,024 mosquitoes have escaped during a given period from the central breeding-pool P, and we divide their subsequent life into 5 stages—the numbers 1,024 and 5 being selected merely for illustration. Rings are drawn around the central pool in order to mark the distance to which the insects may possibly wander up to the end of each stage; and the continuous line shows the course followed by one which has wandered straight onward all its life and has died at the extreme limit to which an insect of its species can generally go, namely, the outermost circle, L. On the other hand, the dotted line shows a course which is likely to be followed by the largest number of the 1,024 insects liberated from the pool—that is to say, a quite irregular to-and-fro course, generally terminating somewhere near the point of origin. The numbers placed on each ring show the number of mosquitoes calculated from the binomial coefficients when $n=5$, which are likely to reach as far as that ring at the time of their death. Thus only 2 out of the 1,024 mosquitoes are ever likely to reach the extreme limit; while, on the other hand, no less than 912, or 89 per cent., are likely to die somewhere within the second ring around the center.

The same reasoning will apply whatever may be the number of mosquitoes liberated from the pool, or the number of stages into which we arbitrarily divide their subsequent life. Suppose, for example, that 1,048,576 mosquitoes escape from the pool and that we divide their life into 10 stages. Then only two of all these insects are ever likely to reach the extreme limit of the outermost circle; only 40 will die at the next circle; only 190 at the next; and so on—the large majority perishing within the circles comparatively close to the point of origin.

This fact should be clearly grasped. The law here enunciated may, perhaps, be called *the centripetal law of random wandering.* It ordains that when living units wander from a given point *guided only by chance* they will always tend to revert to that point. The principle which governs their to-and-fro movements is that which governs the drawing of black and red cards from a shuffled pack. The chances against our drawing all the twenty-six black cards from such a pack without a single red card amongst them are enormous; as are the chances against a mosquito, guided only by chance, always wandering on in one direction. On the other hand, just as we shall generally draw black and red cards alternately from the pack, or nearly so, so will the random movements of the living unit tend to be alternately backward and forward—tend, in fact, to keep it near the spot whence it started. As there is no particular reason why it should move in one direction more than another, it will generally end by remaining near where it was.

But it will now be objected that the movements of mosquitoes are not guided only by chance, but by the search for food. To study this point, take the diagram just given, place a number of pencil dots upon it at random, and suppose that each pencil dot denotes a place where the insects can obtain food—suppose, for example, that the breeding pool lies in the center of a large city and that the pencil dots are houses around it. Consideration will show that the centripetal law must still hold good, because there is no reason why the insects should attack one house more than another. There is no reason why a mosquito which has flown straight from the pool to the nearest house should next fly to another house in a straight line away from the pool, rather than back again, or to the right or left. The same law of chance will continue to exert the same in-

fluence, and the insects will always tend to persecute most those houses which lie in the immediate vicinity of their breeding pool. Even when there are many pools scattered about among the houses, there is no reason why, after feeding, the mosquitoes will go to one rather than to another; and the result must be that in general they will tend to remain where they were.

Self-evident as this argument may now appear, it is not understood by many who write on the subject and who seem to think that mosquitoes radiate from a center and shoot forever onward into all parts of the country as rays of light do. Accepting this fallacy without question, they argue that it is useless to drain local breeding pools because of the influx of mosquitoes from without. Such an influx certainly always exists; but I shall now endeavor to show that it can not generally compensate for local destruction.

Let us consider a tract of country over which numbers of mosquito-breeding pools are scattered, with houses and other feeding places lying among them. Suppose we draw a straight line across this country and drain away all the pools to the right of it, leaving all those to the left of it intact. Then all the insects on the left of the line must be natives of that part; and all those on the right of it must be immigrants which have crossed over the line from the left. How many mosquitoes will there now be on the right side, compared with those on the left side? The following diagram will enable us to consider this question more conveniently.

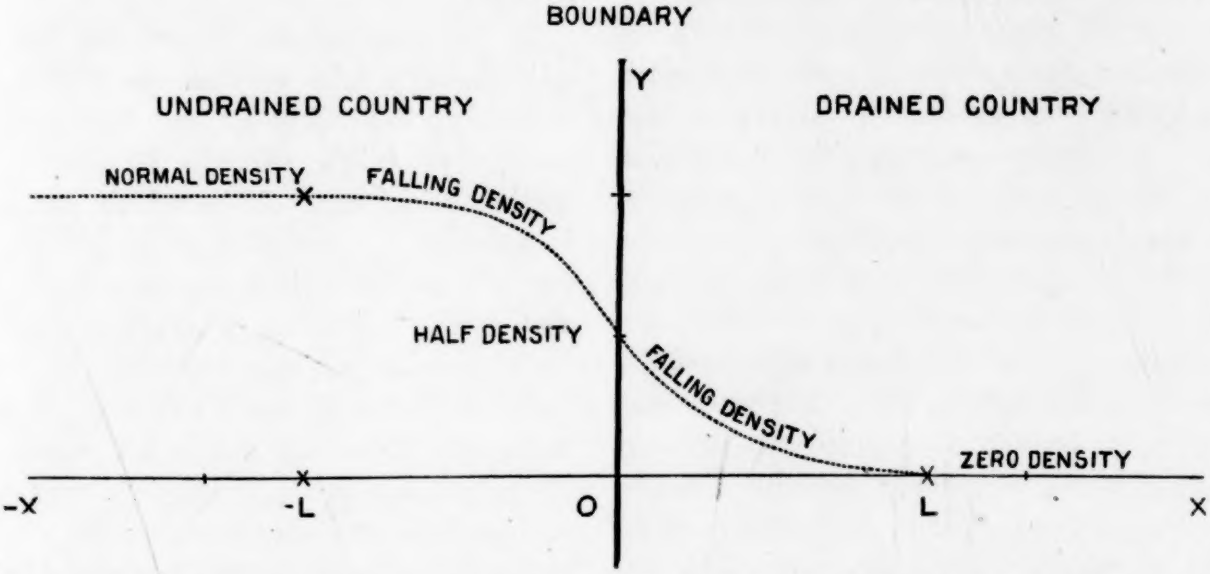

DIAGRAM II. Curve of falling mosquito-density due to drainage on right of boundary. *L* and *-L* are the limit of migration on either side of the boundary.

First, examine the state of affairs before the drainage was effected. We may suppose that mosquitoes were then breeding fairly uniformly over the whole country, and that their density was much the same on both sides of the line. A certain amount of migration across the line, both from right to left and from left to right, must always have been going on; and since the density was equal on both sides, this migration must also have been *equal and opposite*—that is, as many emigrants must have been constantly passing from right to left as from left to right. Now, after the drainage has been effected, the following changes occur. The insects breed as before on the left of the line, and some con-

tinue as before to cross over it into the drained country; but, in the latter, on the right of the line, propagation is entirely checked and, moreover, the migration from it to the left of the line, which used to exist, now ceases. Hence not only must there be a decrease of mosquito-density on the right of the line, due to the local cessation of breeding, but also a decrease on the left of the line, due to the cessation of the migration from the right which formerly took place—that is to say, the drainage has affected the mosquito-density not only up to the line of demarcation, but beyond it. And moreover, since the migration was formerly equal from both sides of the line, it follows that now, after the drainage, the loss on the left side of the line due to the cessation of immigration from the right is exactly equal to the gain on the right due to the continuance of the immigration from the left. That is to say, the mosquitoes gained by immigration into the drained country must be exactly lost by the undrained country. This fact can be seen to be obviously true if we imagine an immense mosquito bar put up along the line of demarcation so as to check all migration across it, when, of course, the mosquito-density would remain as at first on the left, and would become absolute zero on the right: then on removing the mosquito-bar an overflow would commence from left to right, which would increase the density on the right by exactly as much as it would reduce the density on the left.

The dotted line on the diagram indicates the effect on the mosquito-density which must be produced by the drainage. If L is the possible limit of migration of mosquitoes (it may be one mile or a hundred, for all we know), the effect of the drainage will first begin to be felt at that distance to the left of the boundary line. From this point the density will begin to fall gradually until the boundary is reached, when it must be *exactly one half the original density*. This follows because of the equivalence of the emigration and immigration on the two sides. Next, as we proceed from the boundary into the drained country, the density continues to fall, until at a distance L on the right of the line, it becomes zero, the country now becoming entirely free of mosquitoes because they can no longer penetrate so far from the undrained country.

In the diagram the line giving the mosquito-density falls very slowly at first, and then, near the boundary, very rapidly, subsequently sinking slowly to zero. The mathematical analysis on which this curve is based is too complex to be given here; but it is not difficult to see that the centripetal law of random migration must determine some such curvature. The mosquitoes which are bred in the pools lying along the boundary line must remain for the most part in its proximity, only a few finding their way further into the drained country, and only a very few reaching, or nearly reaching, the limit of migration. Though an infinitesimal proportion of them may wander as far as ten, twenty or more miles into the drained country (and we do not know exactly how far they may not occasionally wander) the vast bulk of the immigrants must remain comparatively close to the boundary. And as, for the reason just given, the mosquito-density on the boundary itself must always be only one half the original density, it follows that it must become very rapidly still less, the further we proceed into the drained country. In fact, the analysis shows that the total number of emigrants must be insignificant when compared with the number of insects which remain behind—that is, when they are not drawn particularly in one direction. We are, therefore, justified

in concluding that, as a general rule, the number of immigrants into any area of operations must, for practical purposes, be very small or inappreciable a short distance within the boundary line. The following diagram probably represents with accuracy the effects of thorough suppression of propagation within a circular area.

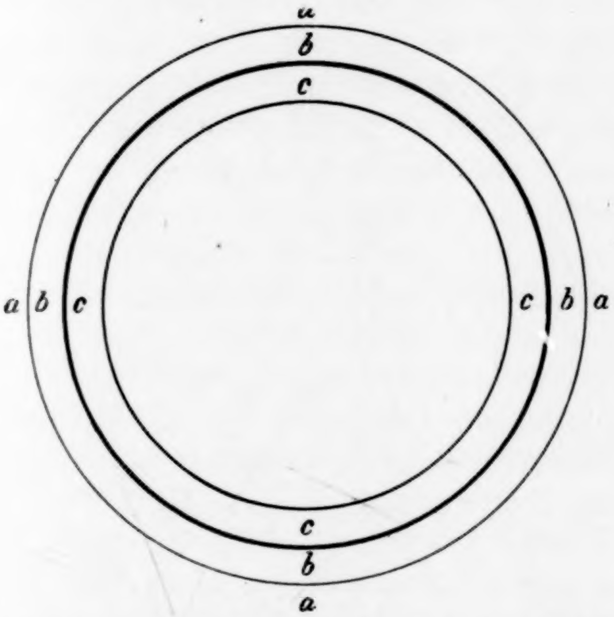

DIAGRAM III. Effect of drainage of a circular area. b = boundary of drained area. Mosquito-density begins to diminish at the circle a; becomes one half at the boundary b; and is small, inappreciable or zero at the circle c.

At the circle (a) and beyond it the mosquito density will be the normal density which existed before the operations were commenced. At (b), the circle bounding the drainage operations, the density will always be about half the normal density. At the circle (c) and within it, the density will be small, inappreciable or zero. The distance from (a) to (b) may be taken as being the same as that from (b) to (c); and, as the mosquitoes penetrating from (b) to (c) must be drawn from the zone between (a) and (b), the *average* result will be the same as if no immigration at all takes place. We do not possess sufficient data to enable us to calculate the actual distance between (a), (b) and (c)—this will depend in a certain measure on the activity of the species of insect concerned and on the existence or absence of special local attractions; but this fact does not discredit the general principles involved.

One case has not yet been considered, namely, that in which there exists only a single feeding place in the whole tract of country—such, for instance, as a single house or group of houses situated in the midst of deserted swamps. In such a case the insects may be compelled to come from considerable distances—from as far as their senses are capable of guiding them—in search of food; and drainage operations carried on with a view to relieving such a house may, for all we know, have to be extended over miles. But such cases are not of great consequence, because drainage is seldom the appropriate measure for isolated dwellings, which can generally be protected at far less cost by means of gauze screens. Moreover, it is very doubtful whether feeding places for mosquitoes are ever so solitary as the case assumes. Where there is one dwelling there are generally many, scattered at various distances over the country; and the insects are known to feed on cattle, birds and other animals. For towns, where anti-mosquito measures are most demanded, our first assumed condition of uniform attractiveness must, as a rule, be the one in force; and in such cases the centripetal law will hold.

The effect of wind required examination. Theoretically, if the insects are supposed always to remain on the wing, wind blowing on a generating pool will merely have the effect of drifting the whole brood to a certain extent in one direction without changing the *relative* positions of the insects to each other. The result would be the same as indicated in Diagram I., except that the generating pool would now be eccentric. If a proportion of the insects

take shelter, the circles of Diagram I. would become ellipses with the generating pool as a focus. In such a case the wind, and especially devious winds, would have a distributive tendency; but it must be remembered that if the insects are scattered further apart their members at a given point must be reduced. A wind which blows mosquitoes into an area must blow others out of it. The net result of devious winds on a circular drained area would be that the mosquito-density is not so much reduced at the center, but is reduced to a greater distance outside the boundary circle —so that the average reduction remains the same. With a wind blowing continuously from one direction, the indication would be to extend the drainage further in that direction. Obviously, wind may scatter mosquitoes; but it can not create them, nor prevent the total average reduction due to anti-propagation measures, as some people seem to think. It is, however, very doubtful whether wind does not really drive or scatter mosquitoes to any great degree. In my experience they are extremely tenacious of locality. Thus *Anopheles* were seldom seen on Tower Hill, a low open hill in the middle of Freetown, Sierra Leone, although numerous generating pools existed a few hundred yards from the top, all around the foot of it, and the winds were often very strong. If a continuous wind can drive mosquitoes before it, then during the southwest monsoon in India they should be driven away from the west coast and massed towards the east coast; but I have never heard that they are at all less numerous on the west coast. I have often seen very numerous mosquitoes on bare coasts exposed to strong sea-breezes, as at Madras. As a rule, they seem to take shelter in the presence of a strong breeze. Instances of their being driven far by winds are frequently quoted, but in my opinion they were more probably bred, in many such cases, in unobserved pools close at hand. The wind-hypothesis is frequently used by municipal officials as an excuse for doing nothing—it is convenient to blame a marsh miles distant for propagating the mosquitoes which are really produced by faulty sanitation in the town itself.

Another and similar statement is often made with all gravity to the effect that mosquitoes are brought into towns in trains, carts and cabs. So they are; but a moment's reflection will assure us that the number introduced in this manner must always be infinitesimal compared with those that fly in or which are bred in the town itself. Moreover, if vehicles may bring them in they may also take them out.

I will now endeavor to sum up the arguments which I have laid before you—I fear very cursorily and inadequately. First I suggested that there must be for every living unit a certain distance which that unit may possibly cover if it continues to move all its life, with such capacity for movement as nature has given it, always in the same direction. I called this distance the limit of migration. It should, perhaps, be called the ideal limit of migration, because scarcely one in many billions of living units is ever likely to reach it—not because the units do not possess the capacity for covering the distance, but because the laws of chance ordain that they shall scarcely ever continue to move always in the same direction. Next I endeavored to show that, owing to the constant changes of direction which must take place in all random migration, the large majority of units must tend to remain in or near the neighborhood where they were born. Thus, though they may really possess the power to wander much further away, right up to the ideal limit, yet actually they always find themselves confined by the impalpable but no

less impassable walls of chance within a much more circumscribed area, which we may call the practical limit of migration—that is, a limit beyond which any given percentage of units which we like to select do not generally pass. Lastly I tried to apply this reasoning to the important particular case of the immigration of mosquitoes into an area in which their propagation has been arrested by drainage and other suitable means. My conclusions are:

1. The mosquito-density will always be reduced, not only within the area of operations, but to a distance equal to the ideal limit of migration beyond it.

2. On the boundary of operations the mosquito-density should always be reduced to about one half the normal density.

3. The curve of density will rise rapidly outside the boundary and will fall rapidly inside it.

4. As immigration into an area of operations must always be at the expense of the mosquito population immediately outside it, the average density of the whole area affected by the operations must be the same as if no immigration at all has taken place.

5. As a general rule for practical purposes, if the area of operations be of any considerable size, immigration will not very materially affect the result.

In conclusion, it must be repeated that the whole subject of mosquito-reduction can not be scientifically examined without mathematical analysis. The subject is really a part of the mathematical theory of migration—a theory which, so far as I know, has not yet been discussed. It is not possible to make satisfactory experiments on the influx, efflux and varying density of mosquitoes without such an analysis—and one, I may add, far more minute than has been attempted here. The subject has suffered much at the hands of those who have attempted ill-devised experiments without adequate preliminary consideration, and whose opinions or results have seriously impeded the obviously useful and practical sanitary policy referred to. The statement, so frequently made, that local anti-propagation measures must always be useless, owing to immigration from outside, is equivalent to saying that the population of the United States would remain the same, even if the birth rate were to be reduced to zero. In a recent experiment at Mian Mir in India the astounding result was obtained that the mosquito-density was, if anything, increased by the anti-propagation measures—which is equivalent to saying that the population of the United States would be increased by the abolition of the birth rate. It is to be hoped that if such experiments are to be repeated they will be conducted by observers who have considered the subject. In the meantime, I for one must continue to believe the somewhat self-evident theory that anti-propagation measures must always reduce the mosquito density—even if the results at Havana, Ismailia, Klang, Port Swettenham and other places are not accepted as irrefragable experimental proof of it.

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SCIENTIFIC BOOKS.

A Text-Book of Physics: Heat. By J. H. Poynting and J. J. Thomson. London, Charles Griffin & Co. 1904. Pp. xvi + 354.

This text-book is the third of a series on general physics by the two distinguished scholars of Birmingham University and of Cambridge. The other two volumes are 'Properties of Matter,' which has already reached a second edition, and 'Sound,' the third edition of which has recently appeared. Two more volumes, on 'Light' and 'On Magnetism and Electricity,' are in preparation. As Professor Poynting says in the preface to

the volume before us, 'The text-book is intended chiefly for the use of students who lay most stress on the study of the experimental part of physics, and who have not yet reached the stage at which the reading of advanced treatises on special subjects is desirable.' With this end in view special attention is given the description of the fundamental experiments and special emphasis is laid upon the various assumptions, and the conditions under which the different theories hold.

It is of interest to note the order of arrangement of the matter in a text-book written by men so well known as teachers as well as investigators. There are in all twenty chapters, and their contents may be outlined as follows: Discussion of temperature; expansion with rise of temperature; quantity of heat, conductivity; conservation of energy; the kinetic theory of matter; change of state; radiation and absorption; thermodynamics, radiation.

A better order for the presentation of the subject of heat could hardly be imagined; and as one reads the chapters it is only at rare intervals that one feels called upon to offer any criticisms or to make any comments which are not most favorable. It may not be amiss to mention as being worthy of special praise the treatment of such subjects as the kinetic theory of matter, radiation, the porous plug experiment, the discussion of various phenomena in meteorology, the spheroidal state, and the theory of the radiometer. The most valuable feature of the book is undoubtedly the exact statement of the various theories and their limitations. Thus, in speaking of the radiometer, the authors say: "The theory is altogether beyond our scope, but the following account of what occurs may give some idea of the action. It is to be remembered that it is an account and not an explanation." Various sentences like this may be found throughout the book, and any student must be impressed with the great care taken to give a true account of both experiments and theory. There is one criticism, rather general in its nature, which may be passed upon the whole book, and that is that too much attention is given experiments and observations of former days at the expense of more modern work. It does not seem altogether advisable to discuss so fully experiments which were incomplete or mathematical laws which have been shown to represent the truth imperfectly. This is specially marked in the chapter on radiation. Again, in the description of certain forms of instruments, care is not taken to explain certain essential features in their accurate use, as, for instance, the Bunsen ice calorimeter. It would have been well, further, in discussing the difficulties of calorimetry to say a few words concerning the instrument perfected by Waterman. In the chapters dealing with the specific heat of water and the mechanical equivalent of heat good, bad and indifferent experiments are all described together, and a student is not told which are the best. If so many experiments and observations are to be described, it certainly would be best for a student to be told which are designed with the greatest care and which are the most trustworthy.

These slight criticisms are not meant in any way to reflect upon the excellent character of the book. As a text-book it stands by itself and should be put in the hands of every student of physics early in his course.

J. S. Ames.

Minnesota Plant Diseases. By E. M. Freeman, Ph.D., Assistant Professor of Botany, University of Minnesota. Report of the Survey, Botanical Series, V. St. Paul, Minnesota, July 31, 1905. Pp. xxii + 432. 8vo.

From time to time, it has been the pleasant task of the writer to notice the publication of the Botanical Survey of Minnesota, and to comment upon the thoroughly satisfactory style of publication adopted by the director, Professor Conway Macmillan, of the University of Minnesota. The volume now before us fully maintains the high standard set by the previous publications in this series. In its paper, type, illustrations and binding, this volume leaves nothing to be desired. As one turns over the pages, he is struck by the uniformly high quality of the illustrations, whether they are cuts from line drawings, or half-tones from photographs. They are all

judiciously selected, well printed, and give one the impression of illustrating the text rather than of adorning the book. This is not the case with all recent books, in some of which one suspects that pretty pictures have been used to add to the attractiveness of the pages, with only remote reference to the text.

We are told in the preface that the chief object of this book is 'to disseminate knowledge of the destructive parasites of the useful plants of Minnesota, to assist all concerned in the cultivation of plants, to a more intelligent and thorough understanding of the habits of these parasites, and to point out established methods of combating such diseases.' In carrying out this plan, the author gives about one half of the book to a general discussion of the nutrition, reproduction, life methods, and parasitism of the fungi, their rôle in plant diseases, their kinds systematically considered, the prevention of diseases, fungicides, spraying, etc. This is followed by a special discussion of diseases of timber and shade trees, timber rots, diseases of field and forage crops, garden crops, orchards and vineyards, green house and ornamental plants and wild plants. In connection with each disease, there are brief but clear suggestions as to preventive or remedial treatment. The volume must at once be in great demand in Minnesota, and, without doubt, the small edition of 2,500 copies will soon be exhausted. It is so valuable a book that it is certain to be in demand wherever there are students of plant diseases, and to meet this demand it should be placed on sale.

CHARLES E. BESSEY.

THE UNIVERSITY OF NEBRASKA.

Sea-shore Life. The Invertebrates of the New York Coast. By ALFRED G. MAYER. New York Aquarium Series, No. I. Published by the New York Zoological Society. 1905.

Dr. Mayer has succeeded in the difficult task of presenting in a readable and popular form a good deal of information regarding the habits and distribution of the lower marine animals of the coast of New York and of Long Island. A simple description of the appearance and structure of most of the forms is given that will suffice for identification. Especially noteworthy are the large number of new illustrations; most of them photographs of the living animals. While these photographs are not all of equal merit, the majority of them are excellent and valuable.

The book of some 200 pages is not intended as a guide to the New York Aquarium, but it is anticipated that many visitors whose interest has been aroused by the fine exhibit at the aquarium will be glad to learn more about the marine fauna of our coast; and a book of this kind will meet such a need. At present, it is true that the animals in the aquarium are largely fishes and a few other vertebrates, but with the completion of the new salt water system that is now being introduced it will be possible to keep alive many of the more delicate invertebrate forms. When this change occurs the first volume of the New York Nature Series will form a useful compendium to the visitor who desires to study the animals in the aquarium as well as to see them.

Two features of Dr. Mayer's book seem to us to be especially noteworthy. In the introductory statement the theory of evolution is presented in a modest and undogmatic spirit, that will recommend itself to most readers. In the second place many references to more special works are scattered through the text, so that the tyro will be able to follow up any special subject that may excite his interest.

The book is clearly printed and presents a very attractive appearance. It ought to prove useful as well as attractive.

T. H. MORGAN.

SOCIETIES AND ACADEMIES.

THE AMERICAN MATHEMATICAL SOCIETY.

THE one hundred and twenty-fifth regular meeting of the American Mathematical Society was held at Columbia University, on Saturday, October 28. The simultaneous meeting of the American Physical Society afforded an agreeable opportunity for the renewal of cordial relations among the members of the two organizations. The attendance at the morning and afternoon sessions of the Mathematical So-

ciety included thirty members. President W. F. Osgood occupied the chair. The following new members were elected: Professor O. P. Akers, Allegheny College; Dr. R. B. Allen, Clark University; Professor Ernesto Cesàro, University of Naples; Lieutenant Colonel A. J. C. Cunningham, London, Eng.; Miss M. E. Decherd, University of Texas; Mr. W. W. Hart, Shortridge High School, Indianapolis, Ind.; Mr. H. N. Olsen, Bethany College; Mr. F. H. Smith, Southwestern Christian College. Twelve applications for membership in the society were received. The total membership of the society is now 503.

A list of nominations for officers and other members of the council was adopted and ordered placed on the official ballot for the annual election at the December meeting. Dr. W. H. Bussey was appointed assistant secretary of the society.

A committee consisting of Professors Maschke, Pierpont, P. F. Smith, H. S. White, and the secretary were appointed to arrange for the summer meeting and colloquium to be held at Yale University in 1906.

The following papers were read at this meeting:

W. B. Carver: 'On the Cayley-Veronese class of configurations.'

James Pierpont: 'Multiple improper integrals.'

Edward Kasner: 'On the geodesics passing through a given point of a surface.'

H. S. White: 'Poncelet quadrilaterals on a curve of the third order and a conic.'

Max Mason and Professor G. A. Bliss: 'A problem of the calculus of variations in which the integrand function is discontinuous.'

G. A. Miller: 'Groups generated by two operators which transform each other into the same power.'

Burke Smith: 'Determination of associated surfaces.'

L. P. Eisenhart: 'Certain triply orthogonal systems of surfaces.'

The next meeting of the society, which will be the annual meeting for the election of officers, will be held at Columbia University, on Thursday and Friday, December 28–29. The American Physical Society and the Astronomical and Astrophysical Society of America will meet at the same place and time.

The Chicago Section of the Mathematical Society will meet at the University of Chicago, on December 29–30.

F. N. Cole,
Secretary.

THE AMERICAN CHEMICAL SOCIETY. NORTHEASTERN SECTION.

The sixty-second regular meeting of the section was held Friday evening, October 27, in the Walker Building, Massachusetts Institute of Technology, with President Norris in the chair. About 250 members and guests were present. The report of the nominating for officers for 1905–6 was accepted.

Professor Wilhelm Ostwald, of Leipzig, Germany, gave a lecture on the 'Development of Chemistry in France, England, Germany and the United States,' in which he said in part, that chemistry had its earliest development in France, but owing to the centralizing methods of Napoleon I., science had always been monarchical in its tendencies in that country. There had always been a central leader at Paris, who played the rôle of 'king' in chemistry; the succession being Lavoisier, Fourcrois, Berthollet, Gay-Lussac, Dumas, Wurtz and Berthelot, the present ruler, with Moissan already elected the next 'king.' The result had been to greatly retard the advance of the science. Berthelot, for instance, had been able to impose his theories on the whole country, so that it was not until recently in French journals that molecular notation had replaced the older equivalent notation. The doctrine of the conservation of energy was first mentioned in a French journal ten years after its discovery, and the same is true of the theory of electrolytic dissociation. Although lately important discoveries have been made by Becquerel, the Curies, etc., this has been done outside of and in spite of the 'system.' In the same way as the science of chemistry has had one person at its head, it has been centralized in one place, Paris, and very little chemical work has been done in the rest of France.

The opposite conditions have existed in England, where individualism has been the rule. Boyle, Priestley, Cavendish, Davy,

Faraday and others were not connected with the government, and had no encouragement or support from it.

In Germany, which consisted of thirty-six separate different countries during the development of chemistry, there has been a large number of centers of science and independent thinkers. At first Germany was far behind France and England. Liebig was the one who brought about the change. His great discovery of the method of laboratory teaching, of personal teaching supplementing mass teaching in lectures, together with the development of research work as a requisite for graduation at a German university, has led to the enormous development of the science of chemistry in Germany, so that at present over one half or nearer three fourths of the chemical investigation of the world is carried on in Germany, all of which is attributable to Liebig's methods.

In America the development of chemistry has been dependent on the development in foreign countries, and foreign methods have been introduced. At present progress is rapid and the signs are hopeful, but the connection between theoretical and applied chemistry is not so well developed as in Germany, where, at Ludwigshafen for instance, there are one hundred and fifty university graduates employed in technical work in one establishment, and the university professors and scientists in works are in close touch. Professor Ostwald illustrated the close connection of theoretical and practical chemistry in Germany by his own valuable discovery of the preparation of nitric acid from ammonia, by catalytic reactions depending on pure physico-chemical theories.

ARTHUR M. COMEY,
Secretary.

THE CHEMICAL SOCIETY OF WASHINGTON.

THE 161st regular meeting was held Thursday evening, November 9, 1905, in the Assembly Hall of the Cosmos Club. Messrs. S. S. Voorhees and L. S. Munson were elected councilors to represent the Washington Section in the American Chemical Society.

The first paper of the evening, entitled 'Polymorphic Forms of Calcium Metasilicate,' was presented by Dr. E. T. Allen.

The results of the investigation were stated as follows:

Calcium metasilicate ($CaSiO_3$) crystallizes in two different forms, the mineral wollastonite which is monoclinic, and an artificial form which is pseudo-hexagonal. These are enantiotropic polymorphs with an inversion point at 1,190°. The artificial form is more stable above this point, therefore to synthesize the mineral of nature, the melt must first be chilled to a glass, and this then devitrified below the inversion point (800° to 900°). Reversion from pseudo-wollastonite to wollastonite does not take place when the two forms are heated together below the inversion point, but this may be effected by the addition of calcium vanadate which dissolves the pseudo form from the solution of which the more stable wollastonite crystallizes. There is scarcely any volume change in the inversion, the specific gravity of the wollastonite being 2.915 and that of the pseudo-wollastonite 2.912. As an inversion temperature is a point at which two solids are in equilibrium, it remains unchanged no matter what solution the mineral may crystallize from. It is only the temperature of crystallization which is affected.

Since neither pseudo-wollastonite nor paramorphs of wollastonite after pseudo-wollastonite are found in nature, it follows that natural wollastonite has always formed below its inversion temperature; and since wollastonite is very characteristic of contact metamorphic zones, the foregoing may have an important bearing on the temperature of contact metamorphism.

The second paper, entitled 'Investigations on the Properties of Wheat Proteids,' was presented by Dr. Joseph S. Chamberlain.

The conclusions drawn were: (1) The washings from gluten determinations contain 35–40 per cent. of the proteids of wheat, of which about 15 per cent. is composed of the glutinous proteids gliadin and glutenin; (2) the cold alcoholic extract of wheat contains, with the gliadin, about 10–12 per cent. of those pro-

teids soluble in dilute salt solutions; (3) the determination of gluten seems less valuable than that of total proteids (from total nitrogen) and the only separation of proteids that seems warranted for analytical purposes is into (*a*) alcohol soluble, and (*b*) alcohol insoluble.

The last paper of the evening, upon 'The Determination of Mercury and Iodine in Antiseptic Soaps,' was presented by A. Seidell. The method described is briefly as follows: The sample of soap is dissolved in acidulated 95 per cent. alcohol and the mercury precipitated from the clear solution by a stream of hydrogen sulphide gas. After filtration, the iodine is determined in the evaporated filtrate by adding a few drops of nitrous acid, shaking out the liberated iodine with chloroform and titrating the chloroformic solution with standard sodium thiosulphate.

A. Seidell,
Secretary.

THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

The 605th meeting was held October 21, 1905, with President Littlehales in the chair.

Mr. F. H. Bigelow gave informally some account of the Spanish Eclipse Expedition, the three parties of which had good weather, and of the opportunities enjoyed on the voyage for meteorological observations by means of kites.

Mr. F. E. Fowle then presented 'The Seeliger-Halm theory of double stars' with lantern illustrations.

According to Seeliger a temporary star is the result of the collision of some dark star with a meteoric cloud. The star is rendered incandescent, a rapidly-expanding chromospheric envelope is formed, and it becomes accompanied by a ring of cosmic particles under its own gravitational sway. Halm shows that the expanding atmosphere may be divided into two parts: that directly between the star and the earth causing the dark band displaced towards the violet, and that part at the sides the bright band in its normal position. The superposition of the spectrum of the ring causes the apparent reversals and the changes in the displacements of the bright line. This accounts for the typical Nova spectrum. The evolutionary process of such a system, with simple modifications, accounts for many of the observed spectrum changes.

Mr. L. A. Bauer spoke of the 'Inauguration of the Magnetic Survey of the North Pacific Ocean by the Carnegie Institution of Washington.' In the prefatory remarks the present status of some of the greater problems of the earth's magnetism was set forth and it was shown that their final solution could not be expected until the completion of an accurate magnetic survey of the oceans as well as of the land, and that, however great this task might appear, it could be accomplished with good system and management, and ample funds, within a period of from ten to fifteen years.

The Carnegie Institution of Washington has undertaken to do its part in the removal of this hindrance to progress in terrestrial magnetism by making an initial allotment of $20,000 to inaugurate a magnetic survey in that portion of the oceanic areas—the North Pacific Ocean—where data are especially scarce; practically only results along one line, passing from New Zealand to the Hawaiian Islands and to Yokohoma, from accurate magnetic observations having been secured thirty years ago by the *Challenger* expedition. The cooperation of existing magnetic institutions is likewise assured through the action of the International Committee on Terrestrial Magnetism and Atmospheric Electricity, which met at Innsbruck, Austria, last September.

A brief summary was given of previous expeditions and then with the aid of lantern slides views were shown of the Carnegie Institution vessel, the fast-sailing wooden *Galilee*, and of the instruments, accompanied by a description of them and of methods in use. The four instruments enable the three magnetic quantities to be observed in duplicate.

In conclusion it was shown that whenever conditions of weather and sea permitted the making of the magnetic observations on equidistant headings of the vessel for a complete 'swing' forward and back, the average results obtained possessed a very high order of ac-

curacy, and that if it were necessary to still further increase the accuracy of the results, this could be done by spending additional time in the observations. When observations can not be secured on a complete 'swing,' but simply on the regular course of the ship, it is not always possible to mathematically control the deviation corrections applied, owing to accidental conditions entering in. While these corrections in the case of the vessel employed are comparatively small as compared with those of other expeditions, they are of sufficient amount to require being taken into account in securing data of the precision requisite for the solution of some of the greater problems referred to above. Considerable time would be saved were it possible to have a vessel entirely non-magnetic so that the question as to corrections to be applied on account of magnetism of any portions of the ship need not be considered.

The results thus far secured by the *Galilee* on her cruises from San Francisco to San Diego and from there to the Hawaiian Islands, as well as some results obtained by the Coast Survey vessel in the Pacific Ocean—the *Patterson*—proved that the latest magnetic charts are systematically in error, as far as the magnetic declination is concerned, to the extent of from one to two degrees, the charts giving too low values of easterly declination. The lines of equal dip appear to be correct on the average within about one third of a degree. The lines of equal horizontal intensity are systematically erroneous to the extent of one twentieth to one thirtieth part of the absolute value—fully ten times the error of the observation—the charts giving too high values. A consideration of the values obtained by the Coast Survey vessels in the Atlantic Ocean, especially between Baltimore and Porto Rico, likewise shows that the intensity charts give values too great by about the same ratio as in the case of that portion of the North Pacific Ocean considered above.

The president followed with some extended remarks on the subject.

THE 606th meeting was held November 4, 1905.

Mr. H. B. Brooks described, by invitation, 'An Efficiency Meter for Incandescent Lamps' developed by Mr. Hyde and himself for use at the Bureau of Standards. The purpose of this is to give a direct reading of the quotient of the watts used by the candle-power; for commercial 16 c. p. lamps this quotient is three to four. A Weston wattmeter is used, but enough extra resistance is added in the shunt circuit (which has normally some 2,000 ohms resistance) to bring the deflection of the needle down to ten times the quotient, as from 64 to 40. Since this resistance must vary with the observed candle-power, part of it is wound on a block of carefully calculated form and a contact piece carried along with the photometer screen cuts out resistance as the candle-power increases. The instrument is reliable to about one per cent.

Mr. W. P. White then presented 'A Thermal Study of the Mineral Wollastonite' made by himself and Messrs. Allen and Wright. This substance, often found in lavas, was studied to get a probable limit to the temperature that the lava had reached.

Calcium metasilicate, $CaSiO_3$, exists in two forms; one, wollastonite, stable below about 1,180°, and a monoclinic form stable above that temperature. If wollastonite is heated, it changes readily to the other form, but to get the reverse change is often a matter of some difficulty. Hence, on cooling a charge of the melted material, if crystallization occurs, as it usually does, above 1,180°, pseudo-wollastonite is found. It is only now and then that the undercooling is great enough to allow wollastonite to crystallize. Wollastonite can be formed readily, however, by chilling melted material so that it becomes glassy at ordinary temperatures, and then heating this glass to a dull red heat. The melting point of the pseudo-wollastonite is 1,512°. Nine determinations on four separate samples showed a maximum variation of $2\frac{1}{2}°$ in the determination of this point.

In locating the inversion temperature in the electric furnace, great help was obtained by the use of control elements which gave simply the furnace temperature and enabled

allowance to be made for its fluctuation. It is well known that thermoelements deteriorate at high temperatures. This results in an incorrect reading and the error depends on the distribution of temperature in the furnace and, therefore, on the amount and nature of the charge which is being examined, etc. Trouble from this source was largely removed by comparing the working elements with standards which were used for so short a time as to hold their values practically unchanged for several months. The comparison must be made under exactly the conditions for which the temperature reading is intended. Thus for best results in the determination of melting points, comparison must be made during the melting. The relative error of a temperature measurement below 1,550° can in this way be brought within half a degree.

One conclusion from the work is that the temperature of lavas where wollastonite is found can not have exceeded 1,163°.

CHARLES K. WEAD,
Secretary.

DISCUSSION AND CORRESPONDENCE.

DR. O. F COOK'S 'SOCIAL ORGANIZATION AND BREEDING HABITS OF THE COTTON-PROTECTING KELEP OF GUATEMALA.'

SOME of the results of the continued work of the United States Department of Agriculture on the ponerine ant, *Ectatomma tuberculatum* Olivier, introduced into Texas for the purpose of aiding in the extermination of the cotton boll weevil, are given in this paper of fifty-five pages in advance of an illustrated bulletin or report on the same subject. Dr. Cook's paper can hardly be passed over without comment, since it displays so many misstatements of fact, such inadequate knowledge of the work that has been done on other species of ants, and such a wilderness of unkempt argument and speculation as to entitle it to high rank as an example of what a scientific essay should not be.

The burden or 'Leitmotiv' of the whole paper is properly sounded in the introduction, which is well worth quoting in its entirety:

In preceding reports treating of the kelep as an enemy of the cotton boll weevil the distinctness of its behavior from that of the true ants has been noted. To avoid in some measure the misapprehension likely to be caused by calling it an ant it seemed desirable to introduce with the insect its distinctive Indian name, *kelep*. In the minds of the natives of Guatemala, the kelep is not a kind of ant, but an independent animal not to be associated with ants. The more we learn about it the more this aboriginal opinion appears justified, not alone because the kelep is a beneficial insect, but because it has a different mode of existence and a different place in the economy of nature.

The popular classification of the social hymenoptera recognizes three types—the ants, the bees and the wasps, the ants being distinguished from the others by the absence of wings. The kelep falls, however, into none of these groups. To call it a wasp or a bee would not misrepresent the practical facts more than to call it an ant. In reality the kelep represents a fourth category of social hymenoptera, as distinct from the other three as they are from each other. Authorities on the classification of the hymenoptera have admitted a rather close affinity between the wasps and the ants, but the kelep differs from both of these groups and approaches the bees in important respects, and especially in those which affect the question of its domestication and utilization in agriculture.

It was naturally supposed at first that the kelep would have the same habits as the true ants which have been associated with it as members of the same family or subfamily, but the differences were greatly underestimated. If the hymenoptera were classified by a taxonomic system consistent with that applied to the higher animals, the kelep would need to be recognized as the type of a new and distinct family. It is, moreover, the first member of its family of which the habits have become known. Under such circumstances it was quite impossible, obviously, to determine in advance whether its habits and instincts would permit its colonization in the United States and its use in agriculture.

The fundamental difference between the ants and the kelep, and that in which the latter resembles the honey bee, lies in the methods of swarming. Among the bees and the keleps swarming results directly in the formation of new colonies, but the swarming of the ants is a distinct biological phenomenon having for its object cross-fertilization. The kelep is completely socialized, like the honey bee, while the ant is not. The keleps and the honey bees live only in communities, while the ants at one stage of their life

history leave the nest and meet the vicissitudes of independent existence as solitary individuals, like the non-social insects. The social organization of the kelep represents a line of development distinct from that of the ants, and shows a relationship with the parasitic and predaceous wasps rather than with the true ants.

Although fresh surprises meet the reader at every turn as he proceeds to read the paper through, he is led to suspect that Dr. Cook, in spite of his fluent style, may at times be unable to say exactly what he means. He evidently wishes to make us believe that the kelep *quâ* dried insect, spitted on a pin, is nothing but a poor ponerine ant, but *quâ* living, nest-building, boll weevil exterminator, is really a creature *sui generis* which the advanced systematist would do well to regard as the sole representative of a distinct family, the Kelepidæ. Here Dr. Cook shows admirable self-restraint, for it might just as well be made the type of a new phylum (Kelepata) or subkingdom (Kelepozoa). At any rate, it is clear that the kelep rises to a dignity analogous to man, whom certain theological taxonomists regard as a poor, though upright primate physically, but as belonging psychically to an entirely different order of being, because he is possessed of the 'free intelligence of the angels.'

Dr. Cook's amazing estimate is attributable to a confusion of ideas concerning certain well-known phenomena among social insects in general and to a lot of inconclusive, not to say slovenly, observations on the kelep in particular. He begins by confounding the nuptial, or marriage, flight and the swarm, or, at any rate, by continually introducing these in his discussion where they do not belong. The nuptial flight is a well-known occurrence in all social insects that have winged males and winged females, in the honey-bees as well as in the ants and termites. Nevertheless, Dr. Cook believes that it is sorely in need of a new name and suggests 'concourse,' a designation as superfluous as it is inept. Swarming, on the other hand, which is peculiar to the honey-bee, is characterized by the old queen leaving the hive with a detachment of workers and establishing a new colony, while the young queen takes her place with the remaining workers. When he comes to consider the possible occurrence of this phenomenon in the kelep, Dr. Cook increases the confusion by failing to distinguish sharply between 'nest' and 'colony.' A single colony of ants may be confined to a single nest, in which case it has been called monodomous by Forel, or it may extend over several nests, in which case it is polydomous. The latter may have several queens distributed among the different nests. The workers of these are on friendly terms with one another and may visit back and forth. Undoubtedly the inhabitants of such nests occasionally become detached from the parent colony and may be regarded as new colonies formed by a process of budding or stolonization. These conditions are well known in such highly endowed ants as our species of *Formica* and *Camponotus* (*F. rufa, sanguinea, exsectoides, C. maculatus* var. *sansabeanus*, etc.). While there is an unmistakable resemblance between this method of colony formation and the swarming of bees, these ants retain in addition the primitive method of founding colonies by single deälated queens.

Now Dr. Cook's confusion of ideas and lack of information are most flagrantly displayed when he comes to present the facts that seem to him to warrant the separation of the kelep from the true ants and ally it with the honey-bees. Having made the interesting observation that a kelep colony will form new nests by sending out detachments of workers and females or of workers alone, he shuts his eyes to the resemblance between these conditions and those of the higher ants, and forthwith jumps to the conclusion that the kelep can not be a true ant, but must be at least as closely related to the honey-bee.[1] Obviously the very opposite is true, since his observations, rightly interpreted, show a closer relationship between the kelep and the higher ants than has been supposed to exist among

[1] "Kelep nests are frequently placed only a few inches apart, the workers of different colonies not being actively hostile. Members of two colonies will forage on the same cotton plant or tree trunk with no signs of animosity" (p. 14).

the Ponerinæ. But this is not all. Because he has never seen a nuptial flight of male and female keleps, he jumps to the further conclusion that it never occurs and that colonies of this ant can not be founded by solitary females. He says at p. 34, 'there is no provision in nature for a solitary kelep.' His whole description of the nesting habits of the kelep discloses nothing to warrant such a gratuitous assumption. As the colonies are small, their nuptial flights would hardly be noticed by the Indians of Guatemala and may, moreover, occur only during certain years or in the twilight or after dark. That they have not been seen in the colonies brought to Texas is even less surprising, as such flights among other species are celebrated only by flourishing colonies, and everything goes to show that Dr. Cook's importations are not in that condition. The large number of males which he finds suggests a high degree of fertility on the part of the workers. It does not, however, indicate colonial prosperity in these ants, but a scarcity of females. Very similar conditions have been observed by Miss Holliday[2] and myself in another ponerine ant, *Pachycondyla harpax* of Texas, which does not form polydomous colonies.

It is, of course, possible that the nuptial flight may not occur in the kelep, that the males may wander about and fertilize the females within the nests, and that new colonies may be formed exclusively by a process of budding or subdivision of preexisting colonies. But if this is true, we should be led to inferences very different from those announced by Dr. Cook. Far from having 'complete socialization' and representing a higher and more economical form of social life, the kelep would seem to be a retrograde, degenerate or, at any rate, highly specialized ant for the reason that just such conditions, at least so far as the suppression of the nuptial flight and intranidal mating are concerned, occur, in all probability, among the parasitic ants like *Anergates, Symmyrmica, Formicoxenus,* etc., and in highly specialized ants like the Dorylinæ and *Leptogenys,* which are either rare or have an unusual mode of life. And far from being a promising trait in an ant introduced for economic purposes, the very opposite would be the case, as seems to be indicated by the flat failure of Dr. Cook's propaganda. It may be best, however, to refrain from all speculation on this matter till we know more about the colonizing habits of the kelep than can be learned from Dr. Cook's desultory statements. There can be no doubt about the fact that isolated fertile females of certain Ponerinæ are able to establish colonies. In the Bahamas I found satisfactory evidence of this both in *Pseudoponera stigma* and in *Odontomachus insularis,* and Dr. Cook is still a long way from having proved that the same method is never adopted by *Ectatomma.*

[2] 'A Study of Some Ergatogynic Ants,' *Zool. Jahrb. Abth. f. Syst.,* XIX., 4, 1903, p. 297, 298.

Additional confusion is introduced by Dr. Cook with a set of new terms. He calls 'an insect colony in which all the eggs are furnished by a single laying queen' a 'strictly determinate organization, that is, it reaches a natural limit after the mother insect dies or ceases to reproduce,' and 'colonies may be called indeterminate when the social economy of the insect is such that a lost queen can be replaced.' "Colonies with more than one egg-producing queen may be called compound indeterminate." All of these distinctions are at the present time not only superfluous, but misleading. According to prevailing theory, all ant, wasp and honey-bee colonies would be determinate, since it is supposed that they can not produce females after the reproductive exhaustion or death of the queen. And, for aught we know to the contrary, the same may be true of the termites. Until we are sure that this is not the case, we gain nothing but confusion by adopting such a classification.

Equally futile is his distinction between the 'social principle of matriarchy' and 'ergatarchy' among the social insects. As a member of a colony, the female ant, wasp or humble-bee is no more a ruler or dominating factor in social life than the queen honey-bee. If the female ant, wasp and humble-bee display great initiative in founding their respective colonies, the female honey-bee displays it by killing rival queens, returning to the hive after the nuptial flight, etc.

The following remarks quoted at random from Dr. Cook's paper show the care with which he has studied the literature of his subject. At p. 9 (foot-note) he says:

With these fungus-cultivating ants and termites, at least, it would seem that a new colony can scarcely be founded by a pair of sexual termites or by a single fecundated female ant unless they carry their domesticated fungus with them. It is possible, however, that in both cases the newly mated insects are adopted and set up in housekeeping and farming by workers of their own species, who bring 'spawn' of the fungi from the older colony with which they are in communication. This might the more readily happen because long subterranean galleries are a prominent feature of the architecture of the fungus-growing insects, both ants and termites.

Although nothing is known concerning the origin of the fungus gardens among termites, von Ihering, in an article[3] which should be known to every botanist, has shown that the colonies of *Atta sexdens* are established by isolated queens and how these insects carry over the fungus from the maternal nest to their own. These observations have been fully confirmed by Goeldi[4] and Huber.[5] At p. 24, Dr. Cook says: 'Copulation has never been observed among termites.' On the contrary, it has been repeatedly observed by at least one observer, Dr. Harold Heath.[6] At p. 19 we find the statement that in '*Leptogenys,* the females, though wingless, are very different from the workers.' Miss Holliday and myself have shown in three different papers that the females of this ant can be distinguished from the workers only by a difference in the size of the abdomen and the enclosed ovaries. At p. 17 we find the following statement:

It does not appear that the keleps have the art of regurgitating food for their larvæ or for each other, but they have, instead, the curious habit of opening their mandibles wide and lapping up drops of nectar, moistened sugar or honey on their mouth-parts. The liquid is thus carried into the nest and dispensed to the other members of the community, old and young. The queen is regularly fed in this way, though in a few instances, the queens of captive colonies came to the surface to eat sugar with the workers.

The mode of expression is varied to read as follows at p. 42:

The kelep does not appear to have the art of regurgitating food as do the true ants, but it is the regular custom of the workers to gather up on their mouth parts large drops of nectar, syrup or honey, which are carried into the nest and freely dispensed to the remaining members of the community, as well as to the queen and larvæ.

To any one familiar with the structure of the mouth-parts of the kelep and with the behavior of ants while they are feeding one another, these statements can only mean that the kelep, like the higher ants, not only ingurgitates liquid food, but feeds the other members of the colony by regurgitation. Here, again, Dr. Cook makes a botch of an interesting observation in his desire to make the kelep out to be a most exceptional creature.

In another part of the paper he shows that this ant also feeds its young with pieces of insect food in exactly the same manner as I have described for other Ponerinæ and some of the higher ants (*Aphænogaster, Pheidole*), and as Janet has shown for *Lasius* and Adlerz for *Tomognathus.* Instead of drawing the natural conclusion that the kelep is allied to both the Ponerinæ and higher ants, Dr. Cook concludes that its relationships are 'with the parasitic wasps rather than with the ants.' It is evident that he will be satisfied with any relationship except the true one. As a matter of fact, every habit which he describes shows that the kelep is nothing more nor less than a ponerine ant. It differs from the Ponerinæ hitherto studied and approaches the higher

[3] 'Die Anlage neuer Colonien und Pilzgärten bei Atta sexdens,' *Zool. Anzeig.*, Bd. 21, 1898, pp. 238–245, 1 fig.

[4] Forel, 'Einige Biologische Beobachtungen des Herrn, Prof. Dr. Goeldi an brazilianischen Ameisen,' *Biolog. Centralbl.*, XXV., 1905, pp. 170–181.

[5] 'Ueber die Koloniengründung bei Atta sexdens,' *Biolog. Centralbl.*, XXV., 1905, pp. 606–619, 625–635, 26 figs.

[6] 'The Habits of California Termites,' *Biol. Bull.*, IV., 2, December, 1902, p. 52.

[7] *Loco citato*, pp. 295–297. 'A Study of some Texan Ponerinæ,' *Biol. Bull.*, II., October, 1900, p. 7; and 'A Crustacean-eating Ant (*Leptogenys elongata* Buckley),' *Biol. Bull.*, VI., 1904, p. 251.

ants in having the power of feeding by regurgitation and of forming polydomous colonies. These conditions merely serve to link the Ponerinæ more closely with the Myrmicinæ, Camponotinæ and Dolichoderinæ. Dr. Cook destroys the value of his own observations by continually using them in support of his perverse speculations. I can see no reason, therefore, for revising my opinion in regard to the taxonomic and economic status of the kelep as expressed in two previous papers in this periodical.[8] Apparently the harder Dr. Cook works to confer exceptional attributes on the kelep, the greater becomes its similarity to other ants, especially to the relatively unplastic Ponerinæ, and hence the less promising it becomes as a subject for agricultural experiment.

The sole result, which, in my opinion, we had a right to look forward to, from all this Corybantic enthusiasm over the introduction of an exotic ant into the United States, was not the protection of the cotton plant from the attacks of the boll weevil, but the production by some well-trained entomologist of a carefully written and illustrated memoir on the structure and habits of a ponerine ant. Under the circumstances and with the funds and facilities at its disposal, this lay well within the competence of the Bureau of Entomology, and may, in fact, be actually under way in the promised report. But assuredly Dr. Howard is not to be congratulated on the kelep articles hitherto published under the auspices of his bureau. We are accustomed to receiving much better work from that quarter.

William Morton Wheeler.

ISOLATION AND THE ORIGIN OF SPECIES.

President Jordan's paper 'The Origin of Species through Isolation'[1] has been read by me with much interest. The following paragraph may be quoted as the caption under which he writes:

In nature a closely related distinct species is not often found quite side by side with the old. It is simply next to it, geographically or geologically speaking, and the degree of distinction almost always bears a relation to the importance or the permanence of the barrier separating the supposed new stock from the parent stock.

It appears to me, however, that the case as stated by him can find scant support of the botanists, to whom it is, I think, easier to find exceptions to the rule, than facts in support of it. The question is, of course, a very complicated one and all who embark on a discussion would fain sound the '*obligato*' of Leonard Stejneger, 'so far as I know.' A few instances drawn at random will suffice at least to throw a reasonable but large doubt upon the factor of isolation and the extent of its effects, as stated by President Jordan, at least so far as plants are concerned, and this doubt should, I believe, obligate us to put the caption cited above into the form of an open question.

Lycopodium complanatum L. and *L. tristachyum* Pursh are two very distinct but closely related species of club-mosses occupying the same range. If we attempt to construct a theory of their origin we are compelled to regard them as genetically related, whatever the mode of origin may have been. These species often grow intermingled in the same habitat, and it was the contrast which they presented under such conditions which forced me to examine them with great care and finally to decide upon their distinction.[2] And if, as has been urged, our eastern North American plant, *L. complanatum,* is not the true European species, the case is strengthened rather than weakened.

In the deserts of the southwest are to be found numerous closely related species of cacti, especially of the genus *Opuntia,* occupying the same habitats and, perhaps, the same ranges. It would be difficult to apply the principle of isolation to these. As an example I may say that there are two distinct but closely related species of the prickly pear type, which I may not, in the present state of their taxonomy, presume to name, distinguishable by their fruits, which are in one species

[8] Science, September 30 and December 2, 1904.

[1] Science, II., 22: 545–562, November 3, 1905.

[2] Lloyd, F. E., 'Two Hitherto Confused Species of *Lycopodium*.' *Bull. Torrey Botan. Club,* **26**: 559–567, November 15, 1899.

globose and dark red, and in the other obconical and yellow and brown, in maturing, respectively. The hybrid character of some individuals must be admitted in theory, but the presence of the two types associated in the same habitat is beyond doubt.

Again, to cite a striking, if sweeping, illustration, it is to be properly appreciated that all the species of the fleshy Euphorbiaceæ are confined to one grand region, and all the analogous species of the Cactaceæ to another. The species composing a genus may not be exactly coextensive, but the ranges of many species may and often do overlap, and are to this extent coincident.

Among the violets, the old species, of Gray's 'Manual,' *Viola palmata,* a few years ago was made to comprehend a number of 'varieties' then so called. These have now been shown to be a number of distinct but closely related species, some of which, at any rate, exist side by side in the same habitats and have the same or similar ranges. The same may be said of the certain yellow, stemmed violets, and of the white violets, *V. lanceolata* and *primulæfolia.*

Two species of *Drosera, rotundifolia* and *longifolia,* are constantly found together and are usually expected to be so found by every collector. Two other species are often associated with them, though not always.

Œnothera biennis and *O. Oakesiana* grow together in the same habitat. So also *Asclepias incarnata* and *pulchra,* two species of the milkweed which are not readily recognized by the tyro, but are yet perfectly distinct, grow constantly associated in the same swamp.

Rhodiola integrifolia is distributed from Colorado to Alaska. In Colorado *R. polygama,* closely related to it, is found with it in the same habitat. The two are not separated by any geographical barrier.

Of the *Potentilla* quite a number of species are very often associated, especially those of the *nivea, gracilis* and *rubicaulis* groups. Three or four of the species may usually be found growing in the same patch of an area not larger than a good-sized room. Of the sedges, many species, often closely related, are to be found in the same bog.

The hammocks of Florida are particularly interesting in this connection because of the great number of species to be found in restricted habitats. Two species of white-flowered *Lantana, odorata* and *involucrata,* are always or nearly always found together, and this applies to the two yellow-flowered kinds, *depressa* and *ovatifolia.* The two live-oaks, *Quercus geminata* and *Virginiana,* are in the same case. *Catopsis nutans* and *Berteroniana* grow intermingled on the same tree, and the same may be said of many other epiphytic species, including Tillandsias, Epidendrons and other orchids. It is worthy of remark that just at the places where the struggle for existence may be presumed to be the most intense between the individuals of the vegetation there also do we find many examples of the same association of closely related species.

Among the ferns, examples are numerous, notably so again in the case of the tropical epiphytic filmy fern, Elaphoglossums and Polypodiums, which grow intermingled, as every one can testify who has examined herbarium specimens which are often composed of two closely related species and therefore not discerned as such by the collector. In Jamaica there are a half-dozen species of tree-ferns which are endemic to the island and are practically always intermingled! Of northern edaphic species *Botrichium obliquum* and *dissectum* are always expected to be found growing together, and the same may be said also of *B. neglectum* and *lanceolatum.* *Dryopteris Goldieanum,* though more restricted in its choice of habitat than *D. marginale,* is often found with it.

Examples among the mosses and liverworts are abundant. *Orthotrichum Brownii, Ohioense* and *brachytrichum* may not infrequently be found mixed together, and even three other species of this genus may be associated with them, all growing on trees. The rocks support, for example, *Dicranum longifolium* and *viridis,* often components of the same tuft, and thus also *D. fuscescens* and *montanum.* Concerning the liverworts, a citation from Spruce is instructive: "I have a small tuft

gathered by Mr. Stabler on Bowfell which comprises five species of *Marsupella* intermingled in the space of a square inch."[3] Be it remembered that Spruce was a most critical student of these forms.

And, if these are proper examples, what of the numerous species of oaks, willows, thorns, asters, golden-rods and many others which spring up in hosts to challenge our scrutiny? Certainly we should think long before applying the principle advocated by President Jordan to these. And what too, shall we say of the many species of the Siphoneæ, the sea-fans, shaving-brushes and their like which grow in the warmer waters of the tropics, many closely related kinds in restricted and identical localities, a condition quite analogous, I venture to say, to the distribution of the oaks, willows, *et cetera*.

It therefore appears that the general law as stated by President Jordan, 'Given any species in any region, the nearest related species is not likely to be found in the same region nor in a remote region, but in a neighboring district separated from the first by a barrier of some sort,' would be more in harmony with the facts in the case as understood by the botanists if stated in the converse form.

President Jordan further admits that, theoretically, mutations may arise which may hold their own in competition with the parent form, but states that such a condition is virtually unknown.[4] This, however, is far from being the case among the plants. Aside from the many properly designated species in cultivation, we have definite, well-authenticated cases of uncultivated forms which give denial.

In 1886, de Vries found in the fields of Hilversum a plant, *Œnothera brevistylis*, which turned out to be a mutant of *O. Lamarckiana*. Although *O. brevistylis* produces comparatively few seeds compared with the parent form, and has not arisen anew as a mutant since the time of its discovery, it has, nevertheless, been able to maintain itself alongside the parent species in the original habitat up till the present time. *O. brevistylis* may be artificially crossed with the parent form and when this is done the progeny split according to the Mendelian principle, so that, even if this were the means of propagation upon which *O. brevistylis* depends, the race would be maintained.

Without recounting the case of *Capsella Heegeri*, and other well-known instances which are completely authenticated, we may see that it is unwise for us to ignore the probability that the same thing has occurred in nature very many times.

The examples which I have given above are only a few of a thousand which might just as easily be recited and have occurred out of hand to me and to a few of my colleagues whom I have questioned on the matter.

Apropos of the proposition[5] that all the organisms in a region unbroken by barriers will slowly change together in the process of adaptation by nature, I may be permitted to point out that it is again still an open question whether this is the method by which a peculiar flora has attained its apparent uniformity. Curiously enough we find markedly desert types, *e. g.*, *Zyzyphus*, a thorny shrub of the desert, growing chiefly along water courses, and opposite types, as *Verbena ciliata*, which can not be seen to differ from a so-called 'mesophytic' garden weed, ecologically or physiologically, getting along quite well in the habitat of *Cereus giganteus*, the ocotillo (*Fouquieria splendens*) and a lot more specialized enough plants. Similarly we find, for examples, a species of *Opuntia*, *O. Opuntia*, growing in our eastern states associated together with mesophytes, just as we find many mesophytes growing in arid deserts. Why? The answer to this query involves some answer to the problem of the origin of desert floras, one, however, which has not yet been solved.

F. E. Lloyd.

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[3] *Rev. Bryologique*, **8**: 104. 1881.

[4] *L. c.*, p. 545.

[5] *L. c.*, p. 547.

THE SMALL MOUNDS OF THE UNITED STATES.

In the two papers on the probable origin of the small mounds in the southern and western parts of the United States, which have ap-

peared in SCIENCE during the present year,[1] the writers have quoted and advanced many and varied theories, none of which, however, appear to be entirely acceptable even by themselves. But why should all these small elevations which evidently occur in large numbers, scattered over widely separated areas between the Mississippi and the Pacific, be considered as having been caused by the same agency? It is impossible to imagine any one natural cause which could have resulted in the formation of all. One theory attributed their origin to glacial action, another considers them to be the work of ants.

Some of the mounds—those in the far northwestern part of the country—may be of glacial origin; if so it should not be a difficult question for a competent geologist to determine. But the same theory can not, of course, be applied to those in the lower Mississippi Valley, for the obvious reason that the glaciers did not extend that far south. Likewise the 'ant hill' theory, when the mounds are considered as a whole, is as equally inapplicable, not only on account of their wide distribution and occurrence far north, but also by reason of the various soil formations of which they are composed. Were they the work of ants some traces or indications of the cavities and passages would certainly be discernible, but such is not the case. The mounds which I have examined in Missouri show no such indications, and Mr. Branner, referring to those on the Pacific coast, writes:

> In California hundreds of mounds have been cut through by railways and by common roads, and many such sections have been examined. The cuttings, being made without any special care exhibit only a compact clayey hard-pan that shows no signs of burrows or anything that has been recognized thus far as different from the soil of the adjacent areas.[2]

Other theories, such as the 'spring and gas vent' and the 'dune,' are without foundation and are scarcely worthy of being mentioned.

Both papers to which I have referred mention the mounds as existing as far north as the Arkansas, but do not allude to the numerous groups which occur in Missouri. These are of a similar form and size and the description of one group appears to be applicable to all.

About four years ago I had occasion to excavate many small mounds that stood on the site of the World's Fair in St. Louis. They formed two groups, one on the ridge, the other not more than six hundred yards distant, was in the lowland on the bank of the small River des Peres. All the mounds of both groups were of a uniform size and were considered as being the same in every respect. But when excavated those on the ridge were found to be ruined habitations. The original surface which served as the floor was readily distinguished. Near the center was the fire bed with ashes and charred wood, worked flint and many small fragments of cloth. Marked pottery were also found on the same level. The mounds of the lower group were likewise examined, but, unlike the others, nothing was found to indicate their origin or use. It will thus be seen that the same theory of origin will not apply to mounds of the same size and appearance when only a third of a mile apart. How unreasonable it is, therefore, to attempt to apply the same theory to those several thousand miles from one another.

I have already mentioned the large groups that exist in Missouri.[3] In Dallas County, in the southern part of the state, they are particularly numerous; many extend in parallel rows along the water courses in the lowlands and others, hundreds, occur in rows on the western slopes, while comparatively few are found on the eastern. Many of these mounds were examined, but nothing was discovered to shed light on their origin; they resembled the lower of the two groups on the fair site, to which I have already referred.

Near the center of one large group of these mounds was one which, although of the same

[1] February 24, 1905, p. 310. A. C. Veatch: 'The Question of Origin of the Natural Mounds of Louisiana, Arkansas and Texas.' Also March 31, 1905, p. 514, 'Natural Mounds or Hog-wallows,' J. C. Branner.

[2] SCIENCE, March 31, 1905, p. 515.

[3] *American Anthropologist*, 1904, p. 294.

size and form, was composed of pieces of limestone, all of which had been carried there. The vegetable mold, the accumulation of a long period of time, had so filled the intervening spaces that the true character of the mound was only revealed when an excavation was made. This mound was between three and four feet in height and about forty feet in diameter. Here we have unquestionable evidence of the work of man. Several other mounds, less than one hundred yards distant, were composed solely of earth and mold similar to the surrounding area.

Probably if these small mounds were not so numerous the question of their origin would never have been raised and they would have been considered, together with the larger mounds, as having been made by man, but the question of number should not influence the decision. It is doubtful if the combined bulk of all these small mounds in the Mississippi Valley is more than equal to that of the one great mound of the Cahokia group.

Without conclusive proof to the contrary, I feel that the most plausible theory of the origin of these small mounds, in Missouri and in other localities where they occur under similar conditions, is that they were made by man, probably to serve as elevated sites for habitations.

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SPECIAL ARTICLES.

THE LOCH LEVEN TROUT IN CALIFORNIA.

In the year 1896 the State Fish Commission of California sent to Captain H. C. Benson, acting superintendent of the Yosemite National Park, five hundred young trout of the species known as Loch Leven trout, *Salmo levinensis,* to be planted in waters of the park. These were placed in a branch of Alder Creek, near Wawona, where they have been allowed to remain undisturbed until the present year.

This Loch Leven trout has been usually considered as a valid species, distinct from the other trout of Great Britain, distinguished from the common brook trout, *Salmo fario,* by the large size, more silvery color, sparsity of spots, the red spots and ocelli characteristic of the brook trout, or brown trout, the trout of Izaak Walton, being usually wanting. The orange edge of the adipose fin, characteristic of the brook trout, is wanting in the Loch Leven trout. The mouth in the latter is said to be smaller, and other differences have been pointed out, but the validity of these structural distinctions has been stoutly denied by Surgeon Francis Day, who has made careful studies of the trout of England.

This fall, Captain Benson caught some fifty-four fishes from the branch of Alder Creek, derived from this plant of Loch Leven trout. These varied from two to seven inches in length, but to his surprise and dismay, he found them corresponding exactly to the markings of the English brook trout, called *Salmo fario,* as shown in the figure published by Mr. W. C. Harris. Four fishes, caught by hand in the brook, he sent to me. They are in fact, so far as one can see, exact representatives in form and color of the common brook trout as seen in the streams of England. The adipose fin is edged with orange. The sides are covered with spots of brown mixed with spots of scarlet, more or less ocellated. These Loch Leven trout in the Yosemite are typical *Salmo fario,* or brown trout of England. Dr. Day speaks of the Loch Leven trout as changing into ordinary brook trout, when planted in streams of Gloucester or Guildford, the colors of the Loch Leven trout being seen on exceptionally well-fed individuals only. In Australia, according to Day, fine examples of the Great Lake trout, *Salmo ferox,* weighing twenty pounds have been reared from eggs of *Salmo fario,* taken in Hampshire and Buckingham. Day also notes that 'a Loch Leven trout having been crossed at Howietoun by a salmon-parr, the offspring possessed the orange-tipped adipose dorsal fin which is seen in the young of the sea trout and the brook trout, and it may be asked from whence had such been obtained unless the Loch Leven possessed the blood of one of these races?' The case is exactly parallel with that of the common trout of Japan, *Salmo masou* Brevoort, which is mature at all sizes from three

ounces to fifteen pounds, and which loses its colors and spots when it enters the sea or when it reaches a large size. Similar changes are shown in each of the four coastwise species of trout of the Pacific coast.

The explanation is apparently this. The trout in Loch Leven is identical as to species with the ordinary brook trout of England. The character of the food supply and of the water of the lake determine its color and appearance. These acquired characters are not hereditary, but are results of conditions in the growth of the individual. The lake trout planted in the brooks grow as other brook trout do. In estuaries of rivers they assume still other characters, and these are equally temporary.

I have no doubt that Dr. Day is right in regarding the large salmon trout of the English bays (*Salmo trutta* L., *Salmo eriox* L., *Salmo cambricus, Salmo albus, Salmo phinoc, Salmo brachypoma*), the golden trout of the estuaries (*Salmo estuarius, Salmo orcadensis, Salmo gallivensis*, etc.), the silvery trout of the various lakes (*Salmo levinensis, Salmo cæcifer*), the great black lake trout (*Salmo ferox, Salmo nigripinnis*), the 'gillaroo,' with the stomach coats thickened (*Salmo stomachicus*), and the common trout of the brooks of northern Europe (*Salmo fario* L., *Salmo ausonii, Salmo gaimardi, Salmo cornubiensis*) as all forms of one and the same species. A member of one of these so-called species would be changed to one of the others if it grew up under the same surroundings. These forms are not subspecies, for that implies a divergence which should be hereditary, however slight. They are, if this view is correct, local variations of one species, for which the oldest name is the half-forgotten one of *Salmo eriox* Linnæus.

A practical question with fish-culturists arises here. "The riparian proprietor," says Dr. Day, "sends for, let us say, *Salmo ferox*, to improve the strain of his local race by crossing, and after a year or two he feels confident that the imported forms are only brook trout. Naturally indignant, he may come to the erroneous conclusion that the purveyor has imposed on him and it will not be until he understands this is a simple variety attaining a large size, due to certain local circumstances, that he will comprehend how his money has been thrown away. He had far better look to the food and condition of the water on his estate before attempting to improve the indigenous breed."

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The Relation of Soil Texture to Apple Production.

The problem of the intelligent selection of an orchard site by a person who contemplates engaging in the production of apples on a commercial scale, or even in a small way, resolves itself into several factors. The climate must be suitable and the physiographic features, including exposure and the attending surface conditions, aeration and drainage, must be favorable. The relation borne to variety by climate, and to a lesser degree by physiographic position, must be carefully determined, for whereas a considerable part of the United States is suited to apple production, certain important varieties, as the Albemarle Pippin, may be successfully grown only in very restricted areas. Other varieties, such as the Baldwin, succeed over a large scope of territory, but still are adapted to only a small part of the general apple belt, while the extent of the range of adaptability of countless other varieties may be said to lie somewhere between those of the two varieties mentioned. Such limitations of variety, however, are known in a general way and with this fund of past experience available the planter need not go far astray in the selection of varieties for his orchard. In the Albemarle area, Virginia, for example, Mooney found that the York Imperial grew to the best advantage in a valley; whereas 'on the eastern side of the Blue Ridge it ripens early, and falls, and does not have as good keeping qualities.'

Again, the Albemarle Pippin[1] thrives on

[1] See Report on the Albemarle Area, U. S. Dept. of Agriculture, Field Operations of the Bureau of Soils, 1902. Report on the Mount Mitchell Area, U. S. Dept. of Agriculture, Field Operations of the Bureau of Soils, 1902. Report on the Bed-

Porter's black loam 'when this type occurs in sheltered mountain coves.' In this case the 'rich, mellow and deep' soil which this variety requires is of little avail when not in conjunction with a sheltered physiographic position which shall so determine the conditions of exposure and aeration as to produce fruit without any cloudiness or imperfection of skin, as either of these conditions detracts to a particular degree from the value of this variety in the somewhat exacting markets to which it goes.

It was also found in the Albemarle area that a loamy phase of the Cecil clay was very satisfactory for Winesap apples, but not as good for the York Imperial, though these varieties seem to do equally well on the Hagerstown loam in the valley of Virginia. If, however, lime be applied to the Cecil clay, the York Imperial is produced as successfully as the Winesap. This indicates one or both of two things: that the soil is acid and that the York Imperial can not overcome this tendency as well as the Winesap, in which case a chemical corrective is required; or that the former variety is more susceptible to the undesirable influences of a soil with texture as stiff as that of the Cecil clay, but that this textural condition is somewhat ameliorated by the application of lime.

Near the southern limit of the apple belt increased elevation so modifies climatic conditions that fruit is successfully produced between the altitudes of 2,500 feet and 3,500 feet in a latitude where at lower levels fruit does not succeed, and within the zone mentioned the climatic relations are further illustrated by the fact that near the lower elevation apples do best in coves on the north side, while in the higher mountains, where it is cooler, south coves are equally good, or perhaps even better, than the north coves.[2]

Other similar specific local problems relating to variety, surface and climatic conditions are not uncommon, and in each instance must be solved for the locality under consideration. Aside from these local qualifications, however, a most important problem which may not be evaded and is of much more general scope remains in the selection of the soil, for upon it depend in large measure the returns from a long-term investment.

In this selection of the soil the ideal to be kept ever in mind is that soil which will produce the greatest quantity of fruit of the best appearance and of the best quality for the longest possible term of years; and he who would be most successful must consider with infinite care not only each of these desiderata in its specific relation to the soil, but also the combined relation of the three to the soil in question. That is, the soil either must be naturally productive or capable of being brought to a high degree of productiveness; its inherent characteristics must be such as to produce fruit not only attractive in appearance when marketed, but also of the highest quality, if the producer is sufficiently far-sighted to recognize that in the long run the greatest profit is to come to him who from the very beginning uses his every endeavor to establish and to maintain permanently the reputation of producing the highest possible quality of fruit demanded, or for which a demand can be created, in the best markets; and finally the soil should be capable of sustaining trees in a profitable bearing condition for a long term of years.

Soils possessing one or two of these characteristics are plentiful, and a large part of the orchards already in existence not only show conclusively certain very desirable traits in such soils as exhibited in growth of tree or in character of fruit, but also indicate the importance of the other qualifications mentioned, if these orchards are to be a commercial success. This is well illustrated, for example, by orchards in the Middle States located on a deep, sandy soil (the Miami sand). This soil is found in a district noted for its successful crops of apples, yet it possesses at most but two of the required attributes. The color of the fruit produced is excellent, and the quality thereof is very good,

ford Area, U. S. Dept. of Agriculture, Field Operations of the Bureau of Soils, 1901.

[2] Report on the Mount Mitchell Area, U. S. Dept. of Agriculture, Field Operations of the Bureau of Soils, 1902.

but the soil is not of such a character as to effect a satisfactory growth of tree, and the bearing life of the tree is so short as to make it manifestly ill-advised to plant orchards upon this type of soil, except possibly to furnish a household supply of fruit.

Similar results observed upon the Norfolk sand and certain light sandy loams, and sandy gravelly loams of the Atlantic seaboard, show clearly that there is a limit in the coarseness of soil texture beyond which the soil can not be used for profitable apple culture. That this limit can be definitely established by further study, particularly if pursued in a comparative way, is, it is believed, unquestionable.

To go to the other extreme of soil texture, many clays and heavy clay loams are either productive or capable of being made so when put into the proper physical condition, and when so improved yield good crops of general farm produce. Apples grown under such conditions, however, as observed on certain fields of the Miami clay loam in Oakland County, Michigan, on the Dunkirk clay in the Champlain Valley, on the Hagerstown clay in Adams County, Pennsylvania, and on local areas of the same type in the Pikeville area, Tennessee, are inclined not only to have greasy skins, but also to be inferior in color, and so fail to command the highest prices. Furthermore, when the subsoil passes a certain stage of stiffness, namely, a texture so close that the roots do not penetrate readily and freely to a depth of several feet, satisfactory growth of tree and consequent quantity of fruit are also impossible.

That these tendencies are largely textural problems is shown on the last two types by local areas containing considerable chert or small stone fragments which serve to loosen the soil. From such areas the fruit has a much clearer and better colored skin, and the greasy characteristic is much less prevalent.

The statement is heard frequently that the apple will succeed on any soil which will produce a good crop of corn. No preconceived idea related to apple growing is more dangerous, perhaps, than this one. That good corn soils may be favorable for apple production is unquestionable, but that the opposite frequently is the case may be best illustrated, possibly, by a concrete example: The best corn soil in the United States which occurs in considerable areas is, probably, the deep, black prairie soil of the middle western states, the Miami black clay loam. But for apples it is found to be favorable only in so far as it conduces to a rank growth of tree. This tendency, indeed, is so marked upon this soil that the yield of fruit is, as a rule, materially lessened, at least until the tree has attained a considerable size. The tree, moreover, is not hardy, the color of the fruit is decidedly inferior, the grain of the fruit inclines towards coarseness, the flavor is never the best, and the keeping qualities are but mediocre. This is, of course, an extreme case, but it represents, nevertheless, a definite tendency of all that class of soils which possess in any marked degree the characteristics of this type.

Having ever in mind the ideal results already defined, as regards quantity and quality of fruit, and the extent to which they are influenced by the character of the soil *per se*, it will be interesting to develop some definite conclusion concerning the soil characteristics which contribute most fully to these ends. Discarding sands and clays for the reasons already noted, there is left a large class of soils ranging at the surface from sandy loams to clay loams, with the subsoils presenting a similarly wide range of variation.

The apple tree under favorable conditions is a vigorous grower, and has an extensive root system. Such a root system can be developed only where the soil particles are of such size and arrangement as not only to allow free root penetration to considerable depths, but also to retain such amounts of moisture as shall be favorable to root growth, and not allow available plant food to be leached away more rapidly than the tree's needs. These two characteristics, however, start from opposite textural extremes and it is only as they approach each other that a satisfactory condition exists. A light sandy subsoil allows free root penetration, but is not sufficiently retentive of moisture and dissolved plant food to supply the needs of the tree. In fact, if we

investigate a series of soils, keeping constantly our problem in mind, we shall find that the essential characteristics—such as moisture supply, retention of plant food and consequent extensive root growth—are all enhanced as the subsoil becomes heavier in texture until the stage is reached where the roots find their progress somewhat hindered mechanically. Beyond this stage of fineness in texture it is ill-advised to go, for diminished returns from the orchard will be sure to follow in proportion as this limit is exceeded. While this point of texture might be fixed theoretically, it is obvious that it may not be so decided from a practical working standpoint, and even if it could be there are probably too few soils of this exact nature in regions possessing the other favorable attributes to supply the apple trade. There are a great many soils, however, whose subsoils are sufficiently near this ideal to bring satisfactory results. Such subsoils range from very heavy sandy loams to clay loams, limited only as already mentioned, thus including the broad class of loams which increase in desirability as they approach the clay loams.

Inasmuch as the subsoils described can be depended upon largely to contain the optimum, or at least favorable, supply of moisture, and to maintain until needed a corresponding concentration of all available plant food, it follows that with them the conditions are supplied to produce a satisfactory growth of tree for a long term of years, provided a sufficient supply of plant food exists in the soil. If the surface soil be too heavy, however, any one or more of several unfavorable results might follow. When the young tree is transplanted from the nursery a great deal depends upon its ability to establish a healthy, normal and extensive root system the first year. This must be done at first within the limits of the surface soil, and is impossible of realization unless that medium is so mellow and non-resisting that the tiny roots and fibrils may be free to develop in all directions. These conditions are manifestly best obtained in soils not heavier than a medium loam nor lighter than a medium sandy loam. Ready drainage of the surface soil, which is also imperative, would be impaired if the soil were too heavy, and the detrimental effect would be apparent not only in the limited growth of tree and its ability to resist disease, but also when the tree should reach its bearing stage, in the coloring of its fruit.

The influence of the character of the soil is again felt, especially in the more northern districts, in the opportune time of the maturity of the fruit. Apples grown on light sandy soil are often ready for picking before the weather is suitable to place them in ordinary storage, while if placed in cold storage the attending expense is much greater than for fruit which matures later. On the other hand, trees grown on clay, or the heaviest clay loams, may continue their growth so late in the season that the fruit does not reach the most desirable state of maturity before it must be gathered, and the trees themselves are not so well prepared to withstand the severities of the winter climate.

The color of the fruit when harvested, furthermore, can be best only when the fruit has reached the proper stage of maturity before it must be picked. It is understood, of course, that no soil can produce highly colored fruit unless the trees are so trimmed and trained as to admit sunlight freely. Assuming that this has been done on all soils alike, and holding our comparison to data gathered under identical, or at least very similar, climatic conditions, then it may be stated that highly colored fruit may be best obtained on soils not heavier than the limit already given. Fruit of excellent color, nevertheless, may be grown on very sandy soils, as was said in connection with that class of soils, but unsatisfactory tree growth more than offsets this desirable characteristic and so eliminates such soils from serious consideration. It is thus seen that the most desirable soils from the color standpoint fall within the range of texture most desirable from the other points of view already considered.

The fact that unsuccessful orchards are frequently seen on the classes of soils already designated as desirable for apple culture most often indicates some form of neglect in methods of culture, including the mechanical con-

dition of the soil, failure to rotate crops where clean cultivation is not followed, lack of proper trimming, failure to control injurious insects, fungus diseases, etc., or that there is insufficient plant food available. Orchards are sometimes seen, however, in which all these external conditions have been carefully attended to, the trees are thrifty, and still the fruit lacks color and quality. This condition involves a chemical problem and usually indicates, as proved in numerous instances, that the supply of available potash is insufficient for the tree's needs—a lack which must be supplied by rendering available the unavailable potash already in the soil, or by the application of further material in an available form.

Another important problem arises at this point, that is, the relation, if any, which exists between diseases of various kinds to which the apple tree or its fruit is subject and the conditions, as related to the soil, under which the trees are grown. Mr. G. H. Powell, of the U. S. Department of Agriculture, stated in an address to the Western New York Horticultural Society, in 1903, that 'at the present time we would say that the practical control of the scald is primarily an orchard problem and depends on cultural conditions that develop the best and most highly colored fruit.'[3] This being the case, it appears that this malady may be avoided, in some measure at least, by selecting soils which, with other things equal, tend to produce 'the best and most highly colored fruit.' It thus seems possible, and indeed probable, that soils in themselves may have a most direct influence upon the character of the tree growth and fruit growth which shall the better enable these to resist certain forces of disease besides the scald.

That the highest quality of fruit should be obtained on a soil which produces a tree neither stunted nor too rank in growth, but normal, well developed and hardy, and consequently productive of fruit the most attractive in appearance, is a natural inference. Sufficient proof of this point, however, is not at present available, but a field of investigation is opened which will become steadily more important as the already noticeable demand for a higher quality of apples increases.

HENRY J. WILDER.

[3] See Proceedings of the 48th Annual Meeting of the Western New York Horticultural Society, 1903.

A CORRECTION OF THE GENERIC NAME (DINOCHŒRUS) GIVEN TO CERTAIN FOSSIL REMAINS FROM THE LOUP FORK MIOCENE OF NEBRASKA.

WHILE in the field during the past summer (1905) the writer sent to Dr. W. J. Holland (director, Carnegie Museum) a preliminary note on certain fossil remains of the family Suidæ from the Loup Fork Miocene of Sioux County, Nebraska. I proposed *Dinohyus hollandi* as the name and asked Dr. Holland if he would kindly look to see if that generic name was preoccupied before publishing the note. In reply Dr. Holland wrote me that *Dinochœrus* 'appears to be a better word,' and that it was not preoccupied. I agreed to the change, but find that the name *Dinochœrus* has been used by Gloger, for a South African hog (*Hand- und Hilfsbuch Naturgeschichte*, I., pp. xxxii, 131, 1841), and, therefore, propose my original name *Dinohyus hollandi* for the fossil remains, which was published in SCIENCE, N. S., Vol. XXII., No. 555, pp. 211–212, August 18, 1905.

O. A. PETERSON.

CARNEGIE MUSEUM,
October 24, 1905.

QUOTATIONS.

ACADEMIC FREEDOM IN JAPAN.

PROFESSOR TOMIZU, most eminent of Japanese authorities on Roman law and professor in the Imperial University, Tokio, has lost his chair, arbitrarily removed by the minister of education, owing to his passionate denunciation of the ministry for the terms which it authorized Japan's representatives at Portsmouth to accept. He is one of a group of seven professors in the university who have been critical of the ministry ever since the war with Russia began.

Professor Tomizu's eminence together with the radical nature of the government's con-

duct, have stirred twenty professors in the university and not a few other teachers to memorialize the minister of state for education. They insist that the competency of Professor Tomizu to hold his chair and his personal character and general conduct are the main points for a minister of education to consider, and not his political opinions from which the ministry and many others may differ. They contend, moreover, that there is nothing in the rules laid down for civil officials, which authorizes the treatment of a man with a university professor's status in such a way.

This appeal represents the convictions of some of the most eminent names in Japan's list of pedagogues and scientists, who, however much they wish a renewal of Professor Tomizu's right status, care even more for the principle involved and the precedent established, a precedent contrary, they believe, to the best educational and political interests of the land. They realize that if Professor Tomizu can be summarily discharged by a minister of education on this issue, they may be discharged at any time on other issues.

In this country academic opinion usually is favorable to peace and hostile to war and extreme measures. In Japan, during the recent conflict with Russia, academic opinion has been conspicuous for a belligerency of spirit.

Japan's surviving autocracy and absolutism under parliamentary forms, has enabled the ministry in its dealing with journalists to be as severe and peremptory as public welfare seemed to make necessary. Professor Tomizu has felt the same iron hand, conserving the interests of peace, at a time when popular feeling has run high and strong.—*The Boston Transcript.*

NOTES ON ENTOMOLOGY.

SEVERAL fascicles of Wytsman's 'Genera Insectorum' have recently been issued; some of much interest to American entomologists. Fascicle 22 treats of the Braconidæ; it is in two parts, of 253 pages and 3 colored plates; it is written by Gy. V. Szépligeti. His classification is, in the main, that of Dr. Ashmead, but he has added several new genera.

Fascicle 23 deals with the Crioceridæ, a group of chrysomelid beetles. M. Jacoby and H. Clavareau are the authors, and the paper contains 40 pages and 5 colored plates. Most of these forms are exotic.

Fascicle 24 is on the subfamily Scutellerinæ of the family Pentatomidæ. It is by H. Schouteden, and occupies 98 pages and 5 colored plates. Most of the species are from the tropics.

Fascicle 25 is by J. Desneux on the Termitidæ or white ants. There are 52 pages and 2 colored plates. He has given a very complete catalogue of the family. His sinking of the many new genera recently created at the expense of the old genus *Termes* is to be highly commended, although he admits that the genus may be divided into six subgenera.

Fascicle 26 is devoted to the Culicidæ, or mosquitoes; F. W. Theobald is the author. There are 50 pages and 2 colored plates. One notices the omission of several species described by Miss Ludlow, and other American entomologists. Apparently ignorant of their identity, Mr. Theobald retains both *Pelorempis* and *Eucorethra* as distinct genera, and even finds characters to separate them in the table.

A USEFUL article is that by Mr. M. T. Cook on the insect galls of Indiana.[1] It includes a general treatment of galls, a catalogue of the Indiana species, with a brief description, and often figure, of the gall, ending with a bibliography. The insects are not described. The enthusiastic author appears, unfortunately, to have but a slight acquaintance with the European literature on cecidii.

MAJOR T. L. CASEY has revised another large group of American beetles; the tribe Pæderini of the family Staphylinidæ.[2] The generic synopses include all American genera, but the specific tables include only the species from the United States. Many of the genera are described as new, and there are many notes on the position of genera, and suggested improvements in the accepted classification.

[1] 'The Insect Galls of Indiana,' 29th Ann. Rept. Dept. Geol. Indiana, 1904, pp. 801–867, 52 figs.

[2] 'A Revision of the American Pæderini,' *Trans. Acad. Sci. St. Louis*, XV., pp. 17–248, 1905.

Several of our species, previously considered identical with European forms, he finds, upon comparison, are distinct therefrom. Over two hundred species are described as new.

IT is not often that catalogues of exotic insects are issued by Americans; therefore, Mr. Levi W. Mengel's catalogue[3] of the Erycinidæ is all the more noteworthy. It is printed in double columns; necessary references, with dates, and synonymy are given; the species are numbered in the genera; there is a full index; in short it is a very useful work to the student of butterflies the world over.

MR. LEWIS's catalogue of the Histeridæ, a family of beetles, will be a great boon to all who wish to study the group.[4] It is a pamphlet of 81 pages, and lists 2,306 species. It appears to be complete, but, unfortunately, there are a few errors in localities and references. Mr. Lewis's collection of these insects is by far the most valuable in the world.

A LARGE treatise on mosquitoes has been published by Professor R. Blanchard.[5] It at once reminds one of Giles's English work, but is not as technical. Part I. treats of the morphology, anatomy, habits, metamorphoses and parasites of mosquitoes. Part II. is a systematic synopsis and list of all the known species. He decides that the proper name of the yellow fever mosquito is *Stegomyia calopus* Meigen, 1818. Part III. relates to the medical phase of the subject. Mosquitoes are considered as agents in malaria, yellow fever, filariasis, and in their probable relation to other diseases. There are chapters on methods of destroying larvæ and adults, of abolishing their breeding-places, of curing the diseases, and finally on rearing and preparing specimens. An appendix includes a list of recently-described species, and a long bibliography. Photographs of Ross, Finlay, Manson and Grassi adorn the pages. Many of the text figures are from Dr. Howard's works.

[3] 'A Catalogue of the Erycinidæ,' Reading, Pa., May, 1905, pp. 161.

[4] 'A Systematic Catalogue of Histeridæ,' by George Lewis; Taylor and Francis, London, 1905.

[5] 'Les moustiques, histoire naturelle et médicale,' Paris, 1905, pp. 673, figs. 316.

AN elaborate book on the Anopheles mosquitoes of India is that by Messrs. S. P. James and W. G. Liston.[6] Part I. treats of the habits, external anatomy, breeding-places and methods of studying this genus of mosquitoes. Part II. consists of technical descriptions of 23 species, arranged in 10 groups. Very sensibly he neglects to make new genera for these groups. A number of larvæ are described and figured, with details. There are many plates, 15 of which are colored and printed on a green background, quite a novel feature in entomology.

AN interesting arrangement of the genera of Vespidæ, or true wasps, is that by A. Ducke.[7] He believes that the nesting-habits is the clew to the natural classification, and tabulates the South American forms on this basis. Some of the older genera are divided, and he has added descriptions of a few new forms. The plate represents the nests of two species of *Charterginus*, showing the opening on the upper side.

DR. W. A. SCHULZ has issued a separate publication under the title 'Hymenopteren-Studien.'[8] It consists of three parts: First, a list of Hymenoptera collected in various parts of North Africa, with notes and descriptions of new forms; second, new genera and species of Trigonalidæ, describing, at great length, several new types from South America; and third, a list of some Vespidæ and Apidæ from the Amazon region, with descriptions of a few new species.

NATHAN BANKS.

BOTANICAL NOTES.

INDEX OF NORTH AMERICA FUNGI.

FOR many years, Professor Dr. Farlow, of Harvard University, has had under preparation an index of the species of North American fungi which should serve as a guide to the more important systematic literature. The

[6] 'A Monograph of the Anopheles Mosquitoes of India,' Calcutta, 1904, 132 pp., many plates.

[7] 'Nouvelles contributions a la connaissance, des Vespides sociaux de l'Amerique du Sud,' *Rev. d'Entom.*, 1905, pp. 5–24, 1 plate.

[8] Leipzig, W. Engelmann, 1905; 147 pp., 13 text figs.

results of these labors are now shown in the first fascicle (Vol. I., part 1) of the 'Bibliographical Index of North American Fungi' which appears as 'Publication No. 8' of the Carnegie Institution of Washington. The author does not include references to merely economic papers, such as those 'on fungicides and other technical and agricultural subjects,' although even these are cited when they contain notes of interest to the systematist. Likewise, papers relating to the physiology and cytology of the fungi are not included (with some exceptions) nor is the literature of the bacteria and saccharomycetes cited.

The arrangement of the genera is alphabetical, with an alphabetical arrangement of the species under each genus. Under each species, the literature is cited in chronological order. As to classification and nomenclature, the author has been conservative, having 'tried as far as possible to avoid changing names in common use for many years.' The 'Sylloge Fungorum' of Saccardo, and the 'Pflanzenfamilien' of Engler and Prantl have been followed as far as possible. While admitting that 'the present classification of fungi is not one which can be called more than temporary,' the author feels that our knowledge of the fungi of the world is not yet sufficient to make it possible 'to form a really natural and scientific system.'

While following the law of priority in regard to specific names, the author 'has no scruples in declining to accept many of the names of older writers which have of late been substituted for more modern names, since, from the vagueness of the descriptions and the crudeness of the illustrations, it is impossible, in the absence of original specimens, to be sure that the species were the same as those to which they have since been applied.'

In this connection, the significant and pertinent remark is made that "it is best not to make too violent attempts to interpret the older mycologists, but to be content with letting the dead bury their dead. The business of reviving corpses has been carried altogether too far in mycology.' Incidentally, he hopes that the next botanical congress will make a list of names of cryptogams which are to be regarded as fixed and exempt from further changes on the grounds of priority.

From remarks in the preface, we infer that the successive parts may be expected to appear without much delay, although it must necessarily take a good deal of time to revise the manuscript and see it through the press. When completed, it will be invaluable to the working botanist, and it is to be hoped that it can be pushed through the press with all possible speed.

THE FERN ALLIES OF NORTH AMERICA.

PROFESSOR WILLARD N. CLUTE has earned the thanks of naturalists of all kinds, from amateurs to professional botanists by bringing out his book, 'The Fern Allies' (Frederick A. Stokes Co., New York), in which, by means of illustrations and non-technical descriptions, he gives a popular account of the plants which are related to the ferns. They include seven families, namely; *Equisetaceae* (14 species), *Lycopodiaceae* (13 species), *Psilotaceae* (1 species), *Selaginellaceae* (12 species), *Salviniaceae* (3 species), *Marsiliaceae* (5 species) and *Isoetaceae* (21 species). There are thus sixty-nine species described in this book, and, since every species is figured at least once, it is easy to see how useful a book this will be for the general reader and the amateur, while at the same time it is likely to prove handy for the professional botanist also. Good keys to the species are given in each family. At the end of the volume is an alphabetical check-list of North American Fern Allies, including many varieties, and this is followed by a simple glossary. The book is well printed and neatly bound, and deserves a wide sale among all classes of plant lovers.

THE GRASSES OF IOWA.

FOUR years ago, the first volume of 'The Grasses of Iowa' appeared as Bulletin No. 1 of the Iowa Geological Survey. That volume was prepared under the joint authorship of Professors Pammel and Weems, of the Iowa Agricultural College, and F. Lamson-Scribner, of the United States Department of Agriculture, and was devoted to a general discussion of the structure, pathology and economic uses

of the grasses. A second volume has now appeared, bearing the date 1904 on the title page, but with a preface dated April 1, 1905. It is also the joint work of several authors, namely, Professor Pammel, C. R. Bell and F. Lamson-Scribner, the two latter of the United States Department of Agriculture. This volume is almost entirely systematic, including descriptions (and usually figures) of about two hundred species and varieties that are native to Iowa or are grown more or less commonly under cultivation. Short chapters on the physiography and geology of Iowa by Dr. H. F. Bain, and the ecological and geographical distribution of Iowa grasses, by Professor Pammel, and a bibliography, close the volume. The two volumes must prove of great value to the farmers of the state, and the second one especially must be helpful to students and others who are interested in the grasses.

It is unfortunate that the public printer should not have done better by these volumes. Paper, type, proof-reading and binding are poor, and are quite unworthy of the text. The authors as well as the people of the great state of Iowa have a right to something much better.

EXPERIMENTS WITH PLANTS.

YEAR by year, one can see that progress is made in the study of plants and their activities. Instead of learning the systematic classification of a plant, alone, as we used to a generation ago, or making out only its microscopic mechanism, as we did later, we are now shown how we may find out what plants and their different organs are doing at different times in their lives. In a suggestive book, 'Experiments with Plants,' Professor Dr. Osterhout, of the University of California, shows teachers how they may ask many questions of plants in such a way as to have them answered by the plants themselves. In ten chapters, the author takes up as many different subjects as follows: the awakening of the seed, getting established, the work of roots, the work of leaves, the work of stems, the work of flowers, the work of fruits, how plants are influenced by their surroundings, plants which cause decay, fermentation and disease, and making new kinds of plants. By a series of simple experiments, usually with simple and often home-made apparatus, the author enables the student to find out a great many things about plants. More than two hundred and fifty illustrations, make still plainer the very clear directions given for making the experiments, and in both, there is evidence of the author's ingenuity in planning devices for experimental purposes.

CHARLES E. BESSEY.

THE UNIVERSITY OF NEBRASKA.

INTERNATIONAL EXPLORATION OF THE NORTH SEA.[1]

IT would be difficult to estimate in terms sufficiently emphatic the vast, momentous importance which attaches to the great international investigations at present being carried out in regard to the fisheries of the North Sea and adjacent waters. At a conference of delegates held at Stockholm in 1890 (at the instance of the King of Sweden and Norway) a general plan for instituting inquiries was drawn up, which it was confidently asserted would undoubtedly lead to the attainment of a better and much more extended knowledge of the natural history of fishes and the influences which regulate their movements to and fro. At a second conference held at Christiania in 1901 a program of work was formulated, to which the several governments acceded, and in the summer of 1902 operations for the great international scheme—the exploration of the sea—were begun.

The nations engaged in the investigations conjointly with Great Britain, include Belgium, Denmark, Finland, Germany, Holland, Norway, Sweden and Russia. Each country, we are told, sends representatives to a central council, which is located in Copenhagen under the presidency of Dr. Walter Herwig, of Hanover, and the vice-presidency of Professor Otto Pettersson, of Stockholm. Every endeavor has been made to ensure that the investigations are carried out in as thorough a manner as possible. The ground of inquiry extends over a very extended sea area, and involves the elucidation and confirmation regarding various points connected with the

[1] The London *Times*.

phenomena of the sea, of which at present we possess a somewhat limited knowledge.

In carrying on the manifold and intricate investigations regarding the hydrographical and fish-yielding mysteries of the sea each country has its own laboratories and scientific staff, and has also at command one or more steamers. In some cases a hired vessel for temporary use is employed (Great Britain has to be content with one); but in others, as in Germany, Russia, Norway, Sweden and Denmark, special steamers have been built, provided with accommodation for a large scientific staff, and elaborately equipped with scientific appliances and fishing gear. At the central laboratory at Christiania, under the direction of Professor Fridtjof Nansen, the hydrographic apparatus required by the different countries is regulated and distributed. The chemical analyses are checked and controlled, and various hydrographical researches of a special and difficult kind are undertaken.

GREEK AT CAMBRIDGE.

The chairman and secretaries of the committee in support of the recommendations of the studies and examinations syndicate making Greek optional in the entrance examination at Cambridge have addressed the following letter to the editor of the London *Times:*

Of the Graces submitted to the senate of the University of Cambridge on March 2, 3 and 4 last, for the confirmation of the report of the Studies Syndicate, Grace 2 was the most important and the most eagerly contested. It was this Grace which directly proposed to make Greek optional in the previous examination.

The number of members of the electoral roll who are members of the senate—that is, practically, the resident members of the senate—was about 600. In the whole constituency there were about 7,000 voters. The votes were—placet 1,055, non-placet 1,557.

A careful analysis of the poll-book gives the following results:

1. Of the residents, 288 voted in favor of the recommendation that Greek should no longer be compulsory in the previous examination; 240 voted against the recommendation—majority of residents in favor of the recommendation, 48.

2. Of the total number of members of the senate who voted, residents and non-residents included, 1,591 were laymen, 1,021 were clergymen.

Of the laymen, 923 voted in favor of the recommendation; 668 voted against it—majority of laymen in favor of the recommendation, 255.

Of the clergymen, 132 voted in favor of the recommendation; 889 voted against it—majority of clergymen against the recommendation, 757.

THE AMERICAN PSYCHOLOGICAL ASSOCIATION.

According to the preliminary announcement issued by the secretary of the American Psychological Association, Professor Wm. Harper Davis, of Lehigh University, the fourteenth annual meeting will be held at Harvard University on December 27, 28 and 29, in affiliation with the American Philosophical Association.

After the formal opening of Emerson Hall, where the associations will meet in joint session on the afternoon of Wednesday, December 27, to hear addresses by President Eliot and Dr. E. Emerson, a formal joint discussion will be held 'On the Affiliation of Psychology with Philosophy and with the Natural Sciences.' Professor Fullerton, President Hall, Professor Münsterberg, Professor Taylor, Professor Thilly and Professor Witmer have consented to speak.

A conference of the association has also been arranged to consider the subject of 'Cooperation between Laboratories and Departments of Different Institutions.' This will also be thrown open for general discussion. It is possible that another discussion, either on a strictly psychological subject or on the content of undergraduate instruction in psychology, will be arranged for.

Luncheon will be served on Wednesday, December 27, by the Harvard Corporation. After the address of the president, Professor Mary Whiton Calkins, of Wellesley College, on Wednesday evening, a general reception will be held at the residence of Professor and Mrs. Münsterberg, and following the presidential address before the American Philosophical Association, by Professor John Dewey, of Columbia University, a joint smoker will be held in the Harvard Union.

Through the kindness of Harvard students of psychology and philosophy, a limited number of dormitory rooms in Cambridge have been placed at the disposal of the Harvard department for assignment to members of the visiting associations. By the courtesy of the Bertram Hall committee and of Radcliffe students, twenty rooms in Bertram Hall, the Radcliffe College dormitory, will be placed at the disposal of women attending the meetings, preference being given to regular members of the association.

SCIENTIFIC NOTES AND NEWS.

SIR JOHN SCOTT BURDON-SANDERSON, formerly Waynflete professor of physiology and regius professor of medicine at Oxford, has died at the age of seventy-seven years.

DR. GEORGE H. DARWIN, F.R.S., Plumian professor of astronomy and experimental philosophy at Cambridge, has been knighted by King Edward.

THE Southeastern Passenger Association has granted a rate of one fare plus twenty-five cents to those attending the New Orleans meeting of the American Association for the Advancement of Science. Other roads will probably make similar arrangements.

THE American Chemical Society will meet in New Orleans in connection with Section C of the American Association for the Advancement of Science from December 29 to January 2, 1905–06. The president of the society is Dr. Francis P. Venable, of Chapel Hill, N. C., the secretary, Dr. William A. Noyes, of the Bureau of Standards, Washington, D. C. The chairman-elect of Section C is Professor Charles F. Mabery, of the Case School of Applied Science, Cleveland, O., the secretary is Professor Charles L. Parsons, of Durham, N. H.

THE Botanical Society of America will meet in New Orleans from January 1 to 4, 1906, under the presidency of Professor R. A. Harper, of the University of Wisconsin. The headquarters are at the Hotel St. Charles, and the sessions will be held in the rooms of Tulane University. Mr. Frederick V. Coville, the retiring president, has chosen as the subject of his address 'Botanical Explorations in Alaska.'

PROFESSOR V. F. BJERKNES, of the University of Stockholm, has arrived in this country to give the course of lectures at Columbia University that has already been announced.

A TESTIMONIAL banquet was given to Dr. Nicholas Senn, at the Auditorium Hotel, Chicago, on November 11, with about seven hundred physicians in attendance. Dr. Joseph D. Bryant, of New York City, presented a gold medallion to Dr. Senn, miniature replicas of which were distributed among those in attendance. On one side of the medallion was a likeness of Dr. Senn; on the other the inscription: "To Nicholas Senn, the Master Surgeon, from his Fellows, November 11, 1905." Dr. L. G. Nolte, of Milwaukee, Wis., presented Dr. Senn with a silver loving cup, given by his former private pupils.

PROFESSOR JACOB REIGHARD, of the University of Michigan, lectured on 'The Habits of Fishes' at the University of Kansas on November 16. This lecture was under the auspices of the Sigma Xi Society of the university. On Friday morning following he gave the convocation address before the students and faculty upon the subject of 'Instincts of Man.' On Friday evening Dr. Reighard was the guest of honor at a reception, giving him an opportunity to meet the faculty of the university.

DR. W. A. NOYES, of the U. S. Bureau of Standards and editor and secretary of the American Chemical Society, lectured last week before the chemical students of Lafayette College, Easton, Pa., on 'The History of the Discovery of the Composition of Water.'

DR. MAXIMILLIAN MAURER has been appointed director of the Meteorological Station of Zurich.

DR. WILHELM WUNSTORF has been appointed geologist in the Berlin Geological Bureau.

PROFESSOR KOEHLER, the president of the Imperial Bureau of Health at Berlin, has retired.

THE members of the council of the Royal Society for the ensuing year, in addition to

the officers, are as follows: Dr. Shelford Bidwell, Sir T. Lauder Brunton, M.D., Professor J. Norman Collie, Ph.D., Professor Wyndham R. Dunstan, M.A., Professor John Bretland Farmer, M.A., Professor Francis Gotch, D.Sc., Dr. Sidney Frederic Harmer, Sir William Huggins, K.C.B., Professor Edwin Ray Lankester, M.A., Dr. John Edward Marr, Mr. George Ballard Mathews, M.A., Mr. Hugh Frank Newall, M.A., Sir William Davidson Niven, K.C.B., Professor John Perry, D.Sc., Professor Ernest Henry Starling, M.D., Professor William Augustus Tilden, D.Sc.

Sir Frederick Treves gave the opening address of the winter series of the Edinburgh Philosophical Institution on October 31, Lord Rosebery presiding.

A general monthly meeting of the members of the Royal Institution was held on November 6, Sir James Crichton-Browne, treasurer and vice-president, in the chair. The special thanks of the members were returned to Mr. Robert Hannah, M.R.I. for his gift of the picture, painted by him, of 'Master Isaac Newton in His Garden at Woolsthorpe, in the Autumn of 1665.' A Christmas course of lectures, adapted to a juvenile auditory, will be delivered at the Royal Institution by Professor Herbert Hall Turner, F.R.S., on 'Astronomy.' The dates of the lectures are December 28, 30, 1905, January 2, 4, 6 and 9, 1906, at 3 o'clock.

The statue of Benjamin Silliman has been removed from its site on the old campus of Yale University, near the library, to a place between the Sloan and Kent laboratories.

On October 13 a bust of the late Professor M. Nencki was unveiled in the chemical department of the Institute of Experimental Medicine, St. Petersburg. Professor Pawlow delivered an address.

A memorial to Theodore Schwamm, regarded as the originator of the cell theory, is to be erected in his native birthplace, Reuss. The sum of $2,500 has already been collected for this purpose, but an additional sum of equal amount is wanted. It is proposed to create a scholarship as part of the memorial.

Professor John Lewis Morris, emeritus Sibley professor of practical mechanics and machine construction at Cornell University, died on November 19, at the age of sixty-three years.

The death is announced of Mr. W. H. Andrews, assistant chemist in the New York State Agricultural Station at Ithaca.

Dr. Gustave Dewalque, formerly professor of geology at Liège, died at Liège on November 3, in his eightieth year.

Dr. E. Oustalet, professor of zoology in the Natural History Museum at Paris, has died, at the age of fifty-one years.

Freiherr von der Goltz, director of the Agricultural Academy at Poppelsdorff and professor of agriculture at the University of Bonn, died on November 6, at the age of seventy-eight years.

The death is also announced of Dr. W. P. Amalizki, professor of geology and paleontology at Warsaw, and of Professor Bernhard Fischer, director of the chemical research laboratory at Breslau.

The late Stephen Salisbury, of Worcester, Mass., has bequeathed the residue of his estate to the Worcester Art Museum, which, it is said, will receive more than $3,000,000. Many other public bequests have been made by the will, including, in addition to $200,000 to the Worcester Polytechnic Institute, some $250,000 to the American Antiquarian Society and $5,000 and a site for a building for the Worcester Natural History Society.

The executive committee of the National Educational Association authorizes the announcement that the forty-fifth annual meeting will be held in San Francisco, Cal., from July 9 to 13, 1906. The lines of the Transcontinental Passenger Association have authorized a rate of one lowest first-class limited fare for the round trip plus $2, National Educational Association membership fee *viâ* direct routes; this provides for going one route and returning another. For tickets routed *viâ* Portland, Oregon, in one direction the rate will be $12.50 higher. The dates of sale will extend from June 25 to July 7, and the return

limit will be September 15. Stop-overs will be allowed west of the Missouri River and St. Paul on both the going and return trips. As has already been announced, the department of superintendence will hold its next meeting in Louisville, Ky., February 27 and 28 and March 1. Superintendent John W. Carr, president of the department, is formulating the program which it is expected will be issued early in December.

THE next meeting for the Australasian Association for the Advancement of Science will be held in Adelaide during January, 1907.

THE next meeting of the German Society of Experimental Psychology will be held at Würzburg on April 10 to 13. Reports will be presented on the following subjects: (1) the relations between experimental phonetics and psychology, by E. Krueger; (2) experimental esthetics, by O. Külpe; (3) the psychology of reading, by F. Schumann; and psychiatry and individual psychology, by R. Sommer.

THE International Congress on Milk Supply will hold its third congress at The Hague in 1907.

AN American Bison Society has been organized in New York City to take steps to prevent the extermination of the buffalo. The New York Zoological Society is prepared to give a herd of buffalos to be placed on the Wichita forest reserve in Oklahoma.

THE Nicholas Senn Club for Scientific Research has been incorporated in Chicago by Drs. Byron Robinson, Orville W. Mackellar and Arthur McNeal.

THE second session of the Graduate School of Agriculture will be held in the summer of 1906 at the agricultural college of the University of Illinois, under the auspices of the Association of American Agricultural Colleges and Experiment Stations and the University of Illinois.

THE Keep Commission is now investigating the Crop Department Bureau of the Department of Agriculture, of which no chief has been appointed since the resignation of Mr. John Hyde. It is said that the bureau may be abolished, its work being divided between the Weather Bureau and the Census Office.

AT the instance of Professor Robert Fletcher, director of the Thayer School of Civil Engineering, and of the president and faculty of Dartmouth College, a series of lectures has been delivered to the engineering students on 'The Economics of Transportation and on Physical Hydrography,' by Professor Lewis M. Haupt, Sc.D., in which it was shown that the annual freight bill paid for overland transportation in the United States, exclusive of waterways, amounted to the enormous sum of $2,600,000,000, and that although the United States has the lowest average tariff per ton-mile in the world, yet the European railways are able to charge from two to three times as much, with greater profits and still compete with this country for the foreign commerce of the world, because of their improved system of waterways. These facts serve to impress the benefits to all classes of carriers and producers resulting from the utilization of water routes for the raw and bulky materials of low values—not yet sufficiently appreciated by traffic managers of this country. The annual saving which might be effected by the betterment of the common roads as feeders was estimated to be enough to pay all the expenses of the government and the desirability of a much more rapid expansion of commercial channels to keep pace with the growth of vessels was forcefully presented.

UNIVERSITY AND EDUCATIONAL NEWS.

BY the will of the late Stephen Salisbury the Worcester Polytechnic Institute receives a bequest of $200,000. This money comes without restrictions of any kind on the part of the testator. In addition to this bequest Mr. Salisbury, at the time of his resignation a few weeks ago from the presidency of the board of trustees, made an additional gift to the institute of $100,000, to be paid immediately.

FORMAL announcement of the $250,000 legacy to the Sheffield Scientific School from the estate of the late M. D. Viets, of Granby, has been made by Professor Russell H. Chittenden, director of the school. The bequest will

be used for the physical, mathematical and general scientific needs of the school.

The late Frank Harvey Cilley, the engineer, has bequeathed the residue of his estate, which will probably amount to $70,000, to the Massachusetts Institute of Technology for the purchase of suitable books, photographs, casts, anatomical models and statuary for the library and gymnasium of the proposed Walker Memorial Gymnasium, or for special lectures on physical culture.

Mr. T. P. Shonts, chairman of the Isthmian Canal Commission, has given to Monmouth College $10,000 as part of the $30,000 needed to obtain an additional $30,000 which Mr. Andrew Carnegie had promised to give the college for a library.

The foundation is being laid for the north wing of University Hall, of the University of Wisconsin, which, when completed, will almost double the class and lecture room capacity of that building. The new wing will be ready for occupancy at the beginning of the next academic year. The regents of the university have purchased the large lot on the corner of State and Park streets opposite the University Library and the Assembly Hall, as a site for the new administration building. The plans for the new building have not been completed as yet and work will probably not be begun until next spring.

The Experiment Station Record states that with the inauguration of a four-year course of study at the agricultural high school of Vienna, the right has been given the school to confer the doctor's degree (*'Doktor der Bodenkultur'*). The course was formerly a three-year one, and there has long been an effort to raise the grade of work done by the school. The present action places it on a par with the universities and technical high schools.

Exchanges state that the Carnegie College of Hygiene at Dunfermline was formally opened on October 4. The course of study is very comprehensive. The college year is divided into three terms of twelve weeks each, and the work is divided into two sections—theoretical and practical—which, in turn, are subdivided. The theoretical includes human anatomy and physiology, personal and school hygiene, theory of movements and teaching, symptomatology in connection with remedial gymnastics and school hygiene, and voice production. The practical course includes (1) educational gymnastics—Ling's Swedish system, (2) remedial gymnastics and massage, students being allowed, under medical supervision, to treat cases; (3) methods of class teaching, students having charge, under supervision, of classes of all ages in the public schools and in the gymnasium; (4) games, dancing and swimming.

The Rev. David H. Buell, professor of physics in Georgetown University, has been elected president of the institution.

Professor H. B. Dates, dean of the Engineering School of the University of Colorado, has accepted a professorship of electrical engineering at the Case School of Applied Science.

Professor Theodore Whittlesey, for some years connected with the department of chemistry, Cornell University, and more latterly adjunct professor of chemistry in the School of Pharmacy of Northwestern University, has recently been appointed adjunct professor of chemistry in Northwestern University.

Mr. F. J. Seaver has been appointed professor of botany in Iowa Wesleyan College.

The following appointments are noted in *The Experiment Station Record:* J. B. Davidson has been elected assistant professor in agricultural engineering at the Iowa College to succeed C. J. Zintheo, who resigned to take up work in farm mechanics in connection with the irrigation and drainage work in charge of the Office of Experiment Stations. E. T. Robbins has been elected to the position of assistant in animal husbandry, to succeed W. W. Smith, who has been elected assistant of animal husbandry at Purdue University. At the North Carolina Station W. F. Massey has resigned his position as horticulturist and will devote himself to editorial work. O. L. Bagley and R. H. Harper, graduates of the class of 1905, have been appointed assistant chemists to the station.

Dr. Otto Stolz, professor of mathematics at Innsbruck, has retired.